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THE

JOURNAL OF SCIENCE.

NOVEMBER, 1882.

I. RECENT STUDIES IN COMPARATIVE PSYCHOLOGY.*

IT is well known that common convention refers the actions of animals other than man to "instinct." What this same "instinct" is, how it operates, how it is to be distinguished from reason, are indeed questions which few persons even attempt to answer, and concerning which scarcely two of these few are agreed. For all this we find the notion obstinately upheld by literary men, lawyers, poets, metaphysicians, and the like—the more obstinately in general the less they have been accustomed to a close and heedful study of animal life. Actual working naturalists, on the other hand, such as have made a point to observe "our poor relations" in the woods, the fields, or, in default and supplementation, in the vivarium or the aviary, almost invariably come to a very different conclusion. They find this phantom "instinct"—in the common acceptation of the term—receding as they approach it, and are driven to admit that animals possess and are guided by faculties like our own in kind, though for the most part of much lower degree. Strange to say, however, the conclusion thus forced upon the naturalist exposes him, if expressed in speaking or in writing, to some little unpopularity. The irrationality of brutes is commonly assumed to be in some mysterious manner a tenet of the Christian faith, and he who shows that they are capable of tracing analogies, of arguing from effect

* Animal Intelligence. By G. J. ROMANES, F.R.S. London: Kegan Paul, Trench, and Co.

to cause, and of recognising an "order of Nature," not seldom finds himself dubbed a "sceptic," a "free-thinker," or at least pronounced "unsound."

We said that concerning the nature of this "instinct" we are as little agreed as were our forefathers a hundred years ago anent the attributes and the functions of phlogiston. The great bulk of non-naturalists—we might almost venture to call them *un*naturalists—hold that instinct, whilst performing to some extent the same functions as those which in us devolve upon reason, and being thus its substitute, is yet a power or faculty of a totally different order. There are thinkers, including a most eminent biologist of Evolutionist principles, who assert that animals remember places, persons, events, &c., not by dint of memory, but of "*quasi*-memory"; that, in like manner, they decide on expedients to be adopted in some emergency, not in virtue of intelligence, but of "*quasi*-intelligence." It may well be asked whether, in thus multiplying imaginary faculties, they are not sinning against Newton's celebrated *regula philosophandi*. A writer in the "Journal of Science" has pointed out that we might as well ascribe the fall of solid bodies upon the moon to "*quasi*-gravitation."

Others, whilst admitting that the actions of animals are mainly instinctive, consider that they are "very commonly tempered with a little dose of judgment or reason."

In the work before us we find an approximate definition of instinct to which little valid objection can be taken. Mr. Romanes regards it as "mental action (whether in animals or human beings) directed towards the accomplishment of adaptive movement antecedent to individual experience, without necessary knowledge of the relation between the means employed and the ends attained, but similarly performed under appropriate circumstances by all the individuals of the same species." We are far from denying that mental action, of the character here laid down, occurs both in man and beast. The conduct of the solitary mason-wasp, or carpenter-bee, in building her nest must clearly be classed as instinctive, thus regarded. She has had no individual experience; the nest is her first effort, and she never survives to construct another. She has had no instruction, since she never saw her parents, who must have perished before she issued from the egg. Her mate and any other of her own species whom she may have seen are exactly in the same position as herself. Hence we need feel little surprise if early observers, who did not and could not enter into the genesis of these powers and aptitudes, saw in them the

workings of some especial unitary faculty, standing in the stead of reason.

But the definition of "instinct" tacitly held by the outside public, and by those whom we may call naturalists of the Old School, is something different from that quoted above. They pronounce it a divinely implanted faculty given to the lower animals instead of understanding. Hence they by implication, at least, denied its presence in man. Now if "instinct" were a divinely implanted faculty, we may safely assume that it would never prove mistaken and misleading. Yet such errors have been observed from the time when Gilbert White described the martins building their nests in the angles of a window too shallow to afford due support to the structure, to the more recent case where Mr. Wallace saw *Rhyncophorus* beetles boring into gummy wood and being held fast by the glutinous fluid.

The believers in "instinct" as a something exclusive of reason will, if they carefully examine the question, find themselves surrounded with difficulties. If they incline to the unitary or monistic view of Nature, they must regard both reason and "instinct"—or, if they prefer the term, "*quasi*-intelligence"—as functions of the brain. But if we take structures so nearly alike as the brains respectively of the higher apes and of man, we may well ask—How can the functions of these organs differ not merely in degree, but in kind? If they suppose—as we believe some do—that the mind of man is a spiritual principle, whilst brutes are totally material, we crave to know how "mere matter" can in the ape perform actions so closely resembling those which in man require the co-operation of matter and spirit? To say that this similarity is divinely ordained is to assume the existence of a "*Deus quidem deceptor*," just as was involved in the old notion of fossils being not the remains of animals once existing, but objects specially created. If, again, we assume that the intelligence of man and the "*quasi*-intelligence" of the ape are both in their essence spiritual, we have then two immaterial principles using tools (*i.e.*, the brain) very similar, producing results very similar, and yet differing not in grade merely, but *toto cælo*. We would commend these questions to the consideration of thinkers.

But if we consider instinct in man or beast to be merely hereditary aptitude, called into play by appropriate circumstances, the difficulties of the case vanish so rapidly that we may expect their entire disappearance at an early date. We see, in fact, new instincts being created in this manner. On this point the well-known case of young pointer dogs is

decisive. Such a dog may never have seen his parents point at birds, and may never have received any training from man; yet when brought for the first time into a stubble field he acts as if he had received such instruction. This case is the more important since it shows us not merely the growth of an instinct by hereditary transmission, but the extinction of a prior instinct of all dogs—that of rushing at any birds or small beasts which make their appearance. Why need we then affect to wonder at the nest-building of a bird or insect? We may even fairly contend that in a creature entirely devoid of intelligence these instincts could never have arisen.

Mr. Romanes, in the work before us, takes the animal kingdom in review from the lowest to the highest forms, and traces everywhere a something which to candid readers must appear to differ from the human mind merely in degree. Of course it is open to the advocates of the old way of regarding the actions of animals to pronounce the capital instances brought forward exaggerations, mis-observations, or even pure inventions. Such a plea is somewhat curious when urged by persons who in other respects are not free from credulity, and who as a body have rarely, if ever, devoted themselves to a close and prolonged study of the habits of animals. They urge that if birds and beasts were rational we should find proofs thereof in their general conduct. But uniform rationality cannot be traced in the actions of mankind: how, then, can we fairly expect to trace it in the doings of the ape or the dog?

The first case given by Mr. Romanes brings into prominence a principle of no small importance. According to a passage quoted from an unpublished MS. of the late Charles Darwin, "Even the headless oyster seems to profit by experience. Dicquemase ("Journal de Physique," xxviii., p. 244) records that oysters taken from a depth never uncovered by the sea open their shells, lose the water within, and perish; but oysters taken from the same place and depth, if kept in reservoirs where they are occasionally left uncovered for a short time, and are otherwise incommoded, learn to keep their shells shut, and then live for a much longer time when taken out of the water." According to Bingley ("Animal Biography," vol. iii., p. 454) this fact is turned to practical account in the "oyster-schools" of France:—"The distance from the coast to Paris being too great for the newly-dredged oysters to travel without opening their shells, they are first taught in the schools to bear a longer exposure to the air without gaping, and when their education

is complete they are sent on to the metropolis, where they arrive with closed shells and in a healthy condition." This simple fact proves that even animals so low in the scale of organisation can modify their actions according to circumstances so as to avoid inconvenience. No possible array of vain words, no hair-splitting, however subtle, can blind us to the truth that we have here intelligence. That an animal to be capable of training must possess intelligence is, indeed, self-evident.

Turning to insects, we find among the ants mental powers of a much higher grade. The evidence that they possess memory is superabundantly complete. Nor do we see by what right their power of remembering, *e.g.*, members of their own community, after prolonged absence, can be called "*quasi*-memory," or can be in any way differentiated from the faculty of memory in ourselves. It is well known that a strange ant, even of the same species, if introduced into a nest, is at once, as a rule, attacked and killed without mercy. But if some of the inmates of a nest are removed, and kept apart from their fellows even for months, they are at once recognised when brought back to the community. It is an important fact that this recognition is not "automatically invariable." "When ants are removed from a nest in the pupa state, tended by strangers, and then restored, some at least of their relatives are certainly puzzled, and in many cases doubt their claim to citizenship. I say some, because, while strangers under the circumstances would have been immediately attacked, these ants were in every case amicably received by the majority of the colony, and it was sometimes several hours before they came across one who did not recognise them."

As to the memory of ants for direction and locality, many of the observations on record are open to doubt on the score that the re-discovery, *e.g.*, of an abandoned nest, as in the case mentioned by Belt, may be accidental. But Karl Vogt, in his "Thierstaaten," mentions that "for several successive years ants from a certain nest used to go through several inhabited nests to a chemist's shop 600 yards distant, in order to obtain access to a vessel filled with syrup." Mr. Romanes adds the very just comment—"As it cannot be supposed that this nest was found in successive working seasons by as many successive accidents, it can only be concluded that the ants remembered the syrup store from season to season."

As regards the power of communication possessed by ants the following experiment, performed by Sir J. Lubbock, is

adduced:—Three glasses were connected with a nest of *Formica niger* by means of three tapes, equal in length and width, and placed parallel with each other, so that the means of access from the nest to each glass were exactly equal. In one of the glasses were placed from 300 to 600 larvæ, in the second two or three only, and in the third none at all. The object of the last was to see whether any ants would arrive at the glasses by mere accident. The experimentalist then put a mature ant into each glass. Each of them took a larva and carried it to the nest, returning for another, and so on. After each journey another larva was put in the glass containing only few larvæ. Sir J. Lubbock reasoned that if ants came to the glasses as a matter of accident, or accompanied one another by chance, or if they merely saw larvæ being brought and concluded that more might be found in the same place, or if the ants were merely guided by the scented track of their forerunners, the number of the ants going to each of the two glasses ought to be approximately equal. But this was not the case. To the glass containing many larvæ there came, in 47½ hours, 257 ants; to that with only two or three larvæ there came, in a time longer by 5½ hours, only 82 ants; and to the glass containing no larvæ there came not one. Hence it is concluded that ants cannot merely tell their companions that there is work to be done, but can give some idea of its quantity, and of the number of hands required for its performance. The precautions taken by Sir J. Lubbock will, on careful consideration, be found to exclude every other interpretation of the facts.

Mr. Romanes also gives certain facts proving that ants can take note of difficulties and dangers, and devise methods how such may be avoided or overcome. They have, indeed, the power of invention. The following incident is quoted from the well-known work of Belt:—"A nest was made near one of our tramways, and to get to the trees the ants had to cross the rails over which the waggons were continually passing and repassing. Every time they came along a number of ants were crushed to death. They persevered in crossing for some time, but at last set to work and tunnelled underneath each rail. One day, when the waggons were not running, I stopped up the tunnels with stones; but although great numbers carrying leaves were thus cut off from the nest, they would not cross the rails, but set to work making fresh tunnels underneath them."

Prof. Leuckart, it is stated, "placed round the trunk of a tree which was visited by ants, as pasture for Aphides, a

broad cloth soaked in tobacco-water. When the ants, returning home down the trunk of the tree, arrived at the soaked cloth, they turned round, went up the tree again to some of the overhanging branches, and allowed themselves to drop clear of the obnoxious barrier. On the other hand, the ants which desired to mount the tree first examined the nature of the barrier, then turned back and procured from a distance little pellets of earth, which they carried in their jaws and deposited one after another upon the tobacco-cloth till a road of earth was made across it, over which the ants passed to and fro with impunity."

A very similar observation was made more than a century ago by Cardinal Fleury, and published by Réaumur:—"The Cardinal smeared the trunk of a tree with bird-lime, in order to prevent the ants from ascending it; but the insects overcame the obstacle by making a road of earth, small stones, &c., as in the case just mentioned. In another instance the Cardinal saw a number of ants make a bridge across a vessel of water surrounding the bottom of an orange-tree tub. They did so by conveying a number of little pieces of wood." These instances, with others that might be either quoted from Mr. Romanes or adduced from other trustworthy sources, prove that ants have the power of invention.

Over a great part of the work before us, as well as the reflections which it cannot fail to suggest, we must pass rapidly. But we are bound to note certain observations showing how rapidly birds in a wild state learn from experience. Mr. Holden found that when telegraph-wires were first set up along the road from Larne to Cushendall, on the coast of the county Antrim, numbers of starlings were found lying dead or wounded on the roadside, they having evidently in their flight in the dusky morn struck against the telegraph-wires. Through the following and the succeeding winters hardly a death occurred among the starlings on their arrival. "It would thus appear that the birds understood the cause of the fatal accidents among their fellow-travellers the previous year, and hence carefully avoided the telegraph-wires; not only so, but the young birds must also have acquired this knowledge and perpetuated it."

Touching the readiness with which birds appreciate a new danger, real or apparent, we were once informed by an old gardener at Aylesbury that a scarecrow is rarely effective for more than three days. At the end of that time the birds have come to the conclusion that it is a mere *brutum fulmen*, and a fresh device is needful.

According to Mr. Thomas Guring the geese kept on the

common outside a small market-town came regularly into the principal street, early in the morning after the fortnightly market-day, to pick up the corn which was scattered from the sample bags. It might be suggested that they were made aware of the market-day by the unusual bustle on the roads, or that they were attracted by the smell of the corn. Accident supplied the *experimentum crucis*. A day of national humiliation was appointed on the day when the market would have been held in the ordinary course of things. There was no traffic on the roads, and no corn to be scented; yet the geese made their usual appearance the next morning. This fact proves that these "stupid" birds can mark intervals of time.

Mr. H. L. Jenkins, speaking of the elephant, states in a private communication to the author:—"There are good reasons for supposing that elephants possess abstract ideas; for instance, I think it is impossible to doubt that they acquire, through their own experience, notions of hardness and weight. The grounds on which I am led to think thus are as follows: a captured elephant, after he has been taught his ordinary duty, is taught to pick up things from the ground and give them to his mahout sitting upon his shoulders. For the first few months it is dangerous to require him to pick up anything but soft articles, because the things are often handed up with considerable force. After a time they appear to take in a knowledge of the nature of the things they are required to lift, and the bundle of clothes will be thrown up sharply as before, but heavy things—such as a crowbar or a piece of iron chain—will be handed up in a gentle manner; a sharp knife will be picked up by its handle, and placed on the elephant's head, so that the mahout can also take it by the handle. I have purposely given elephants things to lift which they could never have seen before, and they were all handled in such a manner as to convince me that they recognised such qualities as hardness, sharpness, and weight."

Mr. Peal observed a young recently-captured elephant, in Assam, pulling up and breaking some bamboo stakes, and carefully examining the pieces. "At last it seized one firmly in its trunk, and, advancing its left fore leg, passed the fragment of bamboo under the arm-pit, and began to scratch with some force. A large elephant leech soon fell to the ground, which, from its position, could not easily be detached without this scraper which had been deliberately made by the elephant." The narrator very truly says that these scrapers are *bonâ fide* tools, "intelligently made for a

definite purpose." Man, we see, is not the only tool-using animal.

Sir E. Tennent, in his "Natural History of Ceylon," gives an account of a wild rogue elephant which had driven two men up a tree. "After vainly attempting to pull up or overturn the tree, observing a pile of timber at a short distance, he brought it all (thirty-six logs), one at a time, to the root of the tree, and piled them up in a regular business-like manner; then placing his hind feet on this pile, he raised the fore-part of his body and reached out his trunk, but still could not touch us." We have here, surely, a rationally conducted siege!

In the section on the domestic cat a capital case is given on the authority of Mr. James Hutchings, of Banbury. "An old tom cat used a young bird, which had fallen out of its nest, as a decoy for the old birds. The cat touched the young bird with his paw when it ceased to flutter and cry, in order that by thus making it display its terror the old cock bird—which was all the time flying about in great consternation—might be induced to approach near enough to be caught. Many times the cock bird did so, and the cat made numerous attempts to catch it, but without success. All the while a kitten had to be kept from killing the young bird." Here we have a stratagem devised for effecting a definite purpose. That stratagem could only have struck a being who had observed and remembered that birds will run unusual risks in the defence of their young. But on careful examination we find here something further. Why did the cat not at once devour the young bird? Because he hoped to employ it as a means for entrapping a more valuable booty —one of the parents. Here, then, we find an instance of an animal repressing its natural instincts, and foregoing a present advantage in order to secure some greater benefit hereafter.

Prof. Mivart ("The Cat," p. 373) enumerates certain "human mental powers, of the possession of which by the cat no evidence exists." Among these figures "A power of, on certain occasions, deliberately electing to act (or to abstain from acting) either with or in opposition to the resultant of involuntary attractions and repulsions — will." Here, then, we have distinct evidence of such "will."

Mr. Romanes represents Arago as having observed the case of a turnspit which refused to make the spit revolve when it was his companion's turn to work. The eye-witness of the incident, if we mistake not, was Ampère, and Arago merely mentions it in his official *éloge* of his deceased friend.

But the incident is a full proof that brutes possess a sense of justice.

A Mr. W. H. Bodley gives an incident which throws a curious light upon the inner life of dogs. He writes concerning a retriever :—" Before he came to me he lived where another dog of similar size was kept, and on one occasion they fought. Having been chastised for this, on future occasions when they quarrelled they used to swim over a river of some breadth, where they could not be interfered with, and fight out their quarrel on the other side. What seems to me noteworthy in this conduct is the *self-restraint* manifested under the influence of *passion*."

Several instances are given of dogs recognising the portraits of their masters,—a faculty which has been denied by writers of the Old School.

Mr. Romanes, as we learn with interest and satisfaction, considers that " the mental life of the Simiadæ is of a distinctly different type from that of the lower animals, and that in their psychology, as in their anatomy, they approach most nearly to *Homo sapiens*." He points out, further, that these animals have never enjoyed the improving influence of hereditary domestication, and regrets that our knowledge of the psychology of the anthropoid apes is exceedingly imperfect. One of the most important parts of the entire work is a full account of observations made on a brown capuchin (*Cebus fatuellus*) which the author had in his house during the winter of 1880-81. Some of the points elicited are exceedingly valuable. The power of monkeys to throw objects at an enemy, and that with great force and precision, is fully established. The monkey in question having got possession of a hearth-brush, unscrewed the handle, and after much study succeeded in screwing it in again. He practised this new accomplishment for some time, and then threw the brush aside. " The desire to accomplish a chosen task seems a sufficient inducement to lead him to take any amount of trouble. This seems a very human feeling, such as is not shown, I believe, by any other animal."

We must here close our brief and fragmentary survey of a work for which we must bespeak the earnest attention of all naturalists. They will find, we think, full proof that the lower animals possess real, and not merely " *quasi*," intelligence. At the same time no one can deny that the subject has been hitherto only " scratched " on the surface, and that much additional study is needed.

II. CRAM AND ITS AMENITIES.

By Frank Fernseed.

AT the risk of hurting, perhaps, the feelings of such of my readers as are well pleased with the peculiar features of the English system of education, I cannot help calling attention to some recent and alarming revelations. Brain-work, it must be conceded, is not *per se* physically injurious. On the contrary, when kept within reasonable bounds and right conditions, it appears distinctly favourable to health and long life. Wœhler has just departed from us at the age of 82; Dumas is a few days older. Chevreul, accidents excepted, promises fair to become a centenarian. Darwin, though shattered by the fearful seasickness which haunted him during the whole of the memorable voyage of the *Beagle*, yet exceeded the proverbial three score and ten years of human life, and was able to carry on his investigations to within twenty-four hours of his death.

But what are the conditions under which brain-work is safe and salutary? Firstly, the brain, like every organ, must be sufficiently mature before it is subject to much exertion. Being probably the latest of all our organs to reach its full vigour, it cannot be prematurely tasked—I will not say overtasked—without injury to it and to the whole system. It is strange that we can grant the unwisdom of working the muscles, tendons, and bones of a horse before they are sufficiently developed, and yet not hesitate to make a premature demand upon the main nervous centre of our children. But a "sanitary reform" in this direction agrees ill with that industrial organisation of society which, according to some thinkers, is the ultimate goal of human progress.

The second great condition under which study is wholesome is freedom from anxiety, hurry, and worry. This condition is admirably illustrated in the career of almost all great investigators of Nature. Wœhler, for instance, contributed no fewer than two hundred and twenty-five memoirs to the scientific journals or to the Transactions of learned societies. Almost all of these papers are of great value, and many of them embody the outcome of months of careful and delicate experimentation. But in no one case was he compelled to finish any of these researches at a certain date under appropriate pains and penalties. Precisely the same

was the case with Darwin: he was able to pursue his inquiries calmly and dispassionately,—able always to take two or three years if his task could not be finished in one, and in no fear of any unpleasant consequences if some idea which he had taken up should lead to nothing.

But if we say to a man, to a youth, or worse still to a child, "You must by a given date reach a certain standard of knowledge, a certain grade of culture, to be judged of in a very summary way, or in default you shall be cut off from the means of earning a livelihood," we place him in the very conditions wherein study becomes unsanitary, even ruinous, and that the more decidedly the more immature the brain. Yet so long as our national system (?) of education is allowed to turn upon preparing for and "passing" examinations, children at our elementary schools, youths seeking admission into the Civil Service, and young men qualifying themselves for the professions, are kept in peril of having their intellects permanently stunted and enfeebled, and their general health injured. At the risk of digressing from my more immediate subject I will here say that the true method for testing the proficiency of a student should be not catastrophic, but, like the great processes of Nature, continuous, "*stetig*."

To return: these evil consequences of hurried, forced work, under the spur of anxiety, are not mere suppositions as to what may be; they are, and have been, actually observed. We may take first the case of children in our primary schools. Mr. Colt-Williams, in his Official Report to the Committee of Council on Education, on the schools which he had examined last year in the counties of Radnor and Hereford, calls especial attention to the overworking of the young pupils, and says, with an especial reference to the proposed extension of the curriculum:—

"All this extra work means extra hours of study with less play, and children do not get too much play now. When one thinks of children of tender years coming in all weathers one and a half or two miles to school to be there by nine in the morning; school till twelve; play and dinner—and how scanty that dinner often is—till two p.m.; school again till four or 4.30 p.m., and then the walk home, in winter often in the dark, home lessons to prepare for next morning, and this for five days in the week for more than nine months in the year; can more be added? And yet this is the daily routine of schools in my district, though we have no singing by note, very little poetry,—that only in Standards 4, 5, and 6,—and no Science. Time will show whether soundness

in elementary subjects will not give way to the smattering of a medley. As it is, I have heard more than once of children passing sleepless nights, and muttering their sums in their dreams. Can such overwork of young brains be a good thing? and is not the proper development of thews and sinews, of muscles and lung power, of greater importance than an aimless murdering of the gems of Shakspeare and Milton? It may seem a bold thing to say, but I can only express my belief that if the present curriculum be extended, and there is consequently a curtailment of recreation, the physical vigour of our country lads and lasses will deteriorate—a serious question when the needs of Army, Navy, and husbandry are considered."

On this significant passage I must remark that play for a child is not a mere luxury which may be cut off at pleasure. It is an essential part of his education, and vastly more important for his future well-being than any amount of literary culture.

No one who has any acquaintance with physiology—a discipline with which the Committee of Council on Education have doubtless not been much troubled—can fail to catch the sad meaning of the "sleepless nights" and "mutterings in dreams" which are here recorded. The sleep of healthy childhood is deep, and often entirely dreamless. Such symptoms when they occur prove but too plainly a serious disturbance of the nervous system. We talk of "payment by results." Are we to include and pay for constitutions undermined and intelligence weakened?

It must also never be forgotten that physical vigour is important, not merely for the Army, the Navy, and for husbandry, but in every calling and every sphere of life. It is time we cast aside the ideals of mediæval asceticism, the emaciated frames, the flabby muscles, the drooping necks, which were supposed to indicate the triumph of mind over matter. We must return to the healthier notions of classical antiquity, and admit that bodily vigour is the essential groundwork for intense mental activity.

It was once proposed by a peer of the realm, formerly, if I mistake not, a successful university coach, and who may therefore not uncharitably be supposed to retain a certain kindness for "cram," that all children should be forced by the time they reached a certain age to "pass" certain "standards." On the feasibility of this suggestion the following facts may throw some light:—Two boys, the sons of a man who had been for more than two years out of employment, attended a certain school. Like all their family, they appeared to be

very inadequately fed, and for two years they made no progress at all. Without any obstinacy or indisposition to learn they seemed unable alike to comprehend and to remember. At last the father obtained employment, the boys were fairly fed, and at once a total change came on. In the next three months they made far more progress than they had done during the foregoing two years. What, in face of such a fact, can be said of the proposal to force children of a given age up to a fixed standard? The under-fed are, I fear, very many, both in town and country; and there are other physical causes which may render some children less able to learn than others. If ever we succeed in obtaining a responsible Minister for Education in place of the Committee of Council, I trust he may be always a physician. It is a fearful thing to let the brains of a nation be played with by men ignorant and careless of their structure, their development, and their functions.

But we must beware of supposing that the evil physical results of our English system of education end with the primary schools. The striplings and young men who are tormented with competitive examinations have brains more mature than have the children of whom we have just been speaking, but in compensation they are more keenly alive to the consequences of the ordeal they are about to face,—an ordeal which, testing stringently verbal memory, glibness of speech, and cool assurance, does not and cannot test originality of thought, suggestiveness, and fertility in resources. These unfortunate young men know what to them will be the result of failure, and they suffer accordingly. Says the "Lancet": "The competitive system as applied to youths has produced a most ruinous effect on the mental constitution which this generation has to hand down to the next and particularly the next but one ensuing. The work of the Civil Service Commissioners is individually and racially destructive."

Is this, again, mere supposition? I have heard of a batch of successful candidates for the Indian Civil Service being before receiving appointments submitted to medical scrutiny, with the result that several were found suffering from albuminuria,—the result of the strain which had been put upon them. I hear on all hands of young men who have "gone up" for examinations, breaking down in health afterwards whether successful or unsuccessful. And who that is brought in contact with our "competition wallahs" at Somerset House or elsewhere can fail to note the want of life and energy which these unfortunates exhibit in every

look, tone, and movement? Surely the price which the nation is paying for the abolition of "influence and favouritism" is too ruinously dear!

It is a common subterfuge with the defenders of the English system of education with its everlasting prizes and certificates, when pressed on the points just mentioned, to say: "Examinations are a poor test, but we cannot devise a better." It is therefore with peculiar satisfaction that I notice certain revelations which have lately been brought to light, calculated greatly to shake the confidence of the public in this supposed test. The disclosures in question occurred in Dublin, and relate primarily to medical education. But in their essence they apply to the courses of study required for all professions and for all positions where a certain standard of attainments is demanded. Within the last twenty years there have sprung up a class of persons technically known as "coaches," "grinders," &c. These persons make it their special business to "prepare" students and candidates for any and every examination which they may require to pass. At first sight it would of course seem to the uninitiated that such a "coach" must have some happy and secret knack of conveying to his pupils a sound acquaintance with the various branches of science or literature in which they are liable to be examined. On further thought it will appear strange that one and the same man, with perhaps a couple of assistants, should be able to teach a multitude of different subjects better than can a body of professors, each of whom has made one science the study of his life. On further prying, the inquirer will find that the duty and the aim of a coach is to teach, not so much any science, as the art of passing an examination in such science. By his success or failure in communicating this art he must stand or fall. Accordingly he studies closely and carefully, not the subjects, but the examiners. He obtains lists of the questions asked year by year, and these he collates and submits to a close analysis. Some questions he finds recur very frequently, and for these he accordingly specially prepares his pupils. Others, he finds, are very rarely put, and these he considers may be safely neglected. A gentleman of this class said recently in evidence: "I closely watch the whole business. I have two things to find out—first the questions, and next the answers. As regards the questions, I examine the man's previous papers very closely. I exclude the questions which have recently been asked. . . . There is one respected gentleman, who happens to be alive, and his number of questions is something under 50. He repeats

these questions; that is to say, he takes a section of those 50 questions each time."

Hence it will easily be seen that any candidate about to be examined by this gentleman must, however much or however little he may know of the subject in general, be prepared with the answers to these fifty questions. But the judicious coach can make a still closer approximation to the mark, and still further narrow the ground upon which his pupils have to concentrate their attention. Most examinations are twofold, partly in writing and partly oral. The coach just quoted has observed that: "The written questions are used for the *vivâ voce* at the next examination, and *vice versâ*. Examiners have an enormous inclination towards that. If I hear that a man has given (certain questions) *vivâ voce* on any given occasion, those questions are very carefully considered for his next paper. I am pretty sure that in his subsequent (verbal) examination he starts with a few fresh subjects, and that when he is tired, towards the end of the day, he will have got over his first selection, and he will think of what he will ask, and the probability is that he will drop on to the last examination paper, because he has considered it carefully and brooded over the answers."

So much for the "positive method" by which the coach ascertains approximately what any given examiner is likely to ask. But the examiners have other idiosyncracies in addition to the mere repetition or non-repetition of questions, and these, also, the coach notes and takes into account. Thus, one examiner may have a fondness for theories, whilst another, on the same subject, adheres more to facts. Everyone has his special predilections for certain phases of his subject, upon which he, consciously or not, insists. What, for instance, would be the fate of a student in chemistry who presented himself for examination at South Kensington without an extensive knowledge of graphic formulæ? Were he as deeply versed in the philosophy of the science as Mendelejeff, did he rival Fresenius in analytical skill, and vie with Baeyer in the power of effecting organic syntheses, still, lacking this wedding garment, he would be ignominiously cast out.

So much, then, for the "positive method," by which it is calculated what an examiner will probably ask. I turn now to the complementary stratagem, known as the "negative method," by which it can be approximately ascertained what the examiner will not ask. This method, says the same coach, is "by no means equally applicable. There is one gentleman whose examinations have extended over sixteen

years. I have a *précis* of fourteen years of his examinations, and the total of his repetitions is under a dozen. I speak in this case of printed papers." In other words, during fourteen years only twelve instances have occurred where a question once asked has been repeated. Hence the coach declares himself "perfectly safe" in excluding those subjects from the preparation of his pupils.

It cannot for a moment be doubted that a student—if we may so call him—thus trained has an enormous advantage over one who has simply obtained the best possible knowledge of the entire subject. The former has merely to grapple with certain select portions of the science in question, whilst the latter must go over the entire ground. Hence, we need not wonder that "all but the very poorest students" have recourse to these private teachers. We see here, also, the full meaning of those memorable words of Professor Huxley,—"We pass and do not know." Is it not time that the public, therefore, should begin to pay less honour to men who have passed, and to seek out, by their performances, men who know? I may seem prejudiced, but had I any appointment to bestow, my first stipulation would be, "no one who has prepared for any examination need apply!"

III. ON THE "RIGID EARTH" PARADOX.

By E. L. Garbett.

A NOTION seems to be abroad that the mathematical exercises of Professors Sir W. Thomson and G. Darwin, on a "rigid" or a "viscous" planet, apply somehow to the actual Earth, and prove some rigidity in her; or even, according to Mr. Proctor ("Knowledge," p. 258), "conclusively dispose of the theory that the Earth is a mere crust, 40 or 50 miles thick, and full of molten lava." Now I submit that he might, without following a line of their calculations, have gathered from their mere statements of results, that these belong only to imaginary globes, and prove no more about the Earth of Nature than do Hampden's ingenious maps on the *flat* theory "dispose of the theory" that she is a globe.

The beginning, so far as I know, of this "rigid" paradox, was a mistake of Sir W. Thomson's, which Sir Edmund Beckett copied into *one* edition of his capital "Astronomy without Mathematics;" that the rate of precession would not be the same in a fluid earth as in a rigid one, and that the actual rate (commonly explained on the assumption of an unyielding solid ball) would not apply to a perfect fluid. Now in the "Philosophical Transactions" for 1880 (p. 464) we find this entirely retracted. Prof. Darwin there italicises, as both his own result and Sir W. Thomson's, that "*The precession of a fluid spheroid is the same as that of a rigid one which has an ellipticity equal to*" the fluid one's. But meanwhile, as they were correcting this slip, an exactly similar one was made about the tides. These are commonly explained on the same easy idea of a rigid ball covered with sea; and everyone may readily infer (though they would be quite wrong) that, if the Earth's interior be as fluid as the sea, both would be deformed alike, and thus, every spot of sea-surface preserving its distance above the bed unchanged, there would be no marine tide. They would err, because it is a thing "not generally known" that the school-book diagrams all exactly reverse, as regards high and low water, the real effects in a general ocean as shallow as our actual ones. The Moon's (or Sun's) pull *direct*, that would, as they show, heap the waters under the pulling body, and pull the Earth from the further sea surface, would thus act were the ocean thousands of miles deep; but as it is only 2 or 3 miles, this effect is vastly excelled and quite masked by another, namely, that the eastward rotary movement, common to the ground and waters, is *retarded* by the moon wherever they are going *from* her meridian, but *accelerated* where they are returning *to* it,—*i.e.*, retarded during the six hours before moonrise or moonset, and accelerated during the six after those events. This heaps them up at moonrise and moonset, making—were the ocean universal and frictionless—high-water at those times, and low-water under the moon: the contrary of what the popular diagrams show. But as these would be correct for an ocean enormously deeper, so are they for Prof. Darwin's "bodily tides." Accordingly the observations of Palmieri, when Vesuvius is most open, find *low-lava* at moonrise and moonset, and *high-lava* following her upper and lower transits. Now when Prof. G. Darwin said, in 1879 ("Phil. Trans.," p. 28), "Unless the viscosity were very much larger than that of pitch, the viscous sphere would comport itself sensibly like a perfect fluid, and the ocean tides would be insignificant," this meant that the

form of the sea-surface would not vary. If the bodily tide (which is high under the moon) and the sea tide (which is low under the moon) were *equal*, it is plain that no spot of sea-surface would be a foot nearer the Earth's centre at one hour than at another. But as we neither do nor can measure our high and low waters from the Earth's centre, but only from their own bed, the observable tides would be just the same as if the ground had none; and whether bodily tides be greater or less than the oceanic, and synchronous or not, the latter will appear the same. Yet both Professors repeatedly say (as in "Phil. Trans.," 1879, p. 2), "The only terrestrial evidence of the existence of a bodily tide in the Earth would be that the ocean tides would be less in height than is indicated by theory," and (p. 30), "it is only possible to judge of the amount of bodily tide by the reduction of the ocean tide." I submit that the detection has never been possible this way. The only terrestrial evidence I can see possible is in volcanic observations like those of Palmieri.

But Prof. Darwin, in the only place where he reasons from a measured fact, namely, the unexplained residuum of the Moon's acceleration ("Phil. Trans.," 1880, p. 481), gets an equation (63), of which he says, "there are obviously two solutions; one of which represents that the Earth is very nearly fluid, and the other that it is very nearly rigid. In the first case, that of approximate fluidity, . . . the semi-diurnal tide [bodily] only lags by the small angle 14′. But this is not the solution which is interesting in the case of the Earth, for we know that the Earth does not behave approximately as a fluid body." (!) Exactly like Hampden, the earth-flattener, with anything he considers to have *one* solution on the theory of roundness. This is not the solution for us, says he, for we know the Earth is not round! The remark I should have thought the only natural one would be—This exactly agrees with what is found at Vesuvius, high-lava but a minute after the Moon's transits; and is the *only* solution interesting us, as we know the Earth never has behaved approximately as a rigid body; and see, alike by pendulum experiments, by dislocating dykes, and by the almost weekly local earthquakes,—narrowly local (by all these far more cogently than by any eruptions),—that she behaves in all points as a very nearly perfect fluid, covered with a scum, and this partly with sea.

The term "crust," applied to the small solid portion, is very misleading, and should be scum; a *crust* conveying inevitably the ideas both of *tenacity* and some ability of *dome-like* self-support, which are both practically absent.

IV. THE INFLUENCE OF LIGHT ON THE COLOURATION OF ANIMALS.

MR. LEWIS, in a paper lately read before the Entomological Society, gave the outlines of a provisional theory of the part due to light in the production and distribution of the colours of animals. Some of his ideas, though they cannot be regarded as demonstrated, are of an exceedingly interesting nature, and deserve attention.

It will of course be admitted that Natural Selection, Sexual Selection, and Mimetism,—though they may weed out individuals whose colours are ill adapted for concealment, or are unattractive to their destined mates, and may, on the other hand, give an advantage to such as happen to be more favoured in these respects,—cannot in themselves produce colouring-matters to be modified. This is easily shown by a reference to the colours prevalent in different groups of animals. We see, *e.g.*, mammals—such as the lion and many of the antelopes—assuming the colour of the deserts which they inhabit. But we do not see any mammalian species which haunt luxuriant grass-lands, thickets, &c., approximating to the green colour there prevailing, as do many birds, reptiles, and insects. We therefore judge that the mammalian system, whilst it is able to ring the changes among greys, blacks, browns, drabs, yellows, fawn-colours, &c., is unable to produce greens, however advantageous such a colour might prove.

From the general absence of colour in the interior of animals, or, in other words, its concentration upon their outer surface, Mr. Lewis argues that light must play a considerable part in the production of the pigments or the structural arrangements which gave rise to such colours.* We cannot, however, admit that colours are invariably absent in the inner parts of animals. Familiar instances to the contrary are the cochineal, the lac, and the kermes insects, the colours given out by certain of the Heteroptera when crushed, the coloured secretions of beetles of the genus *Timarcha*, and the yellow and orange matters emitted by most Lepidoptera in emerging from the pupa.

* We refer here to the peculiar arrangements in, *e.g.*, the plumage of humming birds, which occasion interference colours in the absence of any pigment.

To proceed: Mr. Lewis contends, as we understand him, that the colouration of animals, especially of insects, is due not to physiological processes carried on within the body, but to the mechanical surface action of the light-rays and of the invisible heat-rays. Thus he holds that the most brilliant, and especially the iridescent, colours appear in animals, or in parts of animals, upon which the rays of light fall in constantly varying directions. As an instance he referred to the varied play of colour so often observed on the necks of pigeons, a part which is almost constantly receiving the sun's rays in a varying direction, whether the bird is engaged in its usual circular flights, or walking about, or is even sitting still—the neck being kept almost constantly in motion. Many other instances might here be given which seem to support this notion. Thus the tail and the neck of the peacock undergo a very varied exposure to light. So does the entire plumage of most humming-birds, and the wings of the diurnal Lepidoptera as they flit up and down in their constantly wavering flight. But there are other cases of brilliant colouration in creatures of very sedentary habits. Few groups of birds are more splendidly coloured than the trogons, which sit, dull and sullen, upon boughs in the shady parts of woods. Not a few ground-beetles, which very rarely fly, and which creep mostly on flat surfaces, are also splendidly coloured.

Mr. Lewis further calls attention to the circumstance that insects which creep upon the ground have their underside black or colourless; such as fly, or as creep upon leaves and flowers, are often gaily tinted beneath. As an affirmative instance he mentioned the genus *Calosoma*. *C. sycophanta*, notwithstanding the splendour of its elytra, is black on its underside. But according to Mr. Bates a certain tropical species of *Calosoma*—which Mr. Lewis did not name—passes its life upon trees, and is brilliant upon both sides. Yet there is a British species of the same genus, *C. inquisitor*, which is chiefly found upon oak trees, but is below of as sombre a tint as the terrestrial species.

Mr. Lewis may appeal with confidence to the difference in the colouration of the upper and lower surfaces traceable in two closely allied groups, the Cicindelidæ and the Carabidæ. The former—of which our common tiger-beetle (*C. campestris*), to be seen on any fine day during the spring and early summer at the upper extremity of Epping Forest, is a typical example—are during all the sunny hours of the day in rapid and unceasing motion, taking short flights, alighting for a moment on the heath or the gravel, and then again

soaring away like a flash of green light. Now, with very few and doubtful exceptions, the insects of this form are nearly as gaily coloured on the abdominal as on the dorsal surface. In the Carabidæ, and especially in the true Carabs, we notice a complete reversal of these conditions. Many of the species in question are unable to fly. This, indeed, is universal in the case of the great genus *Carabus* proper.* Their movements upon the ground are much less rapid and frequent than those of the tiger-beetles. In accordance with the difference in the locomotive powers and habits of the two groups, we find the Carabs, even when brilliant upon their upper surface, dull and dark-coloured beneath. As common examples of this we may take *Carabus nitens*, *C. auratus* (so common in France), *C. auronitens*, *C. pyrenæus*, &c.

Mr. Lewis may claim another affirmative instance for his law in the Buprestidæ. These rank among the most volatile of all beetles. In cloudy weather they are rarely to be found, and in the sunshine, even in Europe, they spend much time in the air, and take wing at once if approached carelessly. In warm climates they even flit up and down without apparent motive, dancing in the air with almost the ease of gnats. And in accordance with these great powers of flight they are generally admitted to be the most gorgeously coloured animals known. Not only so, but their abdominal surface is often as bright and as iridescent as their elytra, and as the pronotum. There are indeed a few exceptions: *Sternocera chrysidiodes* (Madras) is black underneath; so also are *Cyria imperialis* (Australia), *Julidomorpha Bakewellii* (Adelaide), *Stigmodera grandis*, *S. macularia*, and *S. semicincta* (New South Wales), as also *S. nasuta* (Victoria). But so few are these exceptions that we feel at liberty to ask whether there may not be in their habits some anomaly which makes them thus deviate from the bulk of their near allies. It must also not be forgotten that some of the Buprestidæ are sombre both above and below, whilst a few are even more brightly coloured below than above.

* We have never seen a satisfactory suggestion as to the absence or the mere rudimentary condition of the wings in this fine group. In accordance with the doctrine of Natural Selection the wings must have gradually become abortive because found useless, or inconvenient, but as far as the habits of these creatures are known this is hard to understand. Multitudes of them perish at present in floods, whilst if they had wings they might escape the danger. In populous countries great numbers of them are crushed in the early morning by the feet of workmen and others. The possession of wings would enable the insects to avoid this peril. Being also beasts of prey, the power of flight would give them chance of securing booty which must now escape them.

Next, perhaps, in beauty to the Buprestidæ—and if gifted with less command of wing, yet haunting flowers, and thus of necessity much exposed to the sun—come the Cetoniadæ, of which our common rose-beetle (*Cetonia aurata*) serves as a familiar type. Here also there are very numerous instances of a colouration on the lower surface of the insect rivalling or surpassing that of the upper. Among the exceptions rank *Doryscelis calcarata* (Madagascar), where the under surface is black, whilst the elytra and pronotum are bright orange. Upon the whole it may be said that the results of an examination of this family tell in favour of Mr. Lewis, the upper and the lower surfaces being about equal in beauty. We have not, in the necessarily restricted survey we have been able to make, met with any distinct case of a Cetonian displaying iridescent colours above, and being of a black, a deep brown, or grey underneath.

Mr. Lewis does not seem to have mentioned the interesting fact that both among the Buprestidæ and the Cetoniadæ a few species have deep violet, bronze-green, &c., wings, though in the vast majority of beetles these organs are as colourless as those of a house-fly or a wasp. Careful observation of the habits of these species may perhaps throw light on the cause of this anomaly. It will be remembered that in warm climates a great number of the Hymenoptera have beautifully coloured wings.

Mr. Lewis, if we did not mistake him, referred also to the *Phyllophaga*, or, as they might be familiarly called, the cockchafer group, as being instances favourable to his views. This is strictly correct. Many of these insects, which are good flyers, are given to swarming over the tops of the trees and bushes on which they prey. Many of them have not only brilliant colours, but in such forms as *Catalpa*, *Euchlora*, *Macraspis*, &c., the lower surface is often equal, and often even superior to the upper in colouration.

Other groups of the Coleoptera, however, supply much less favourable, or at least less decisive, evidence. If we turn to the common large dung-beetles of Britain, belonging to the genus *Geotrupes*, we find them black above, and richly coloured with violet, blue, deep green, and bronze underneath. But this, according to the principles laid down by Mr. Lewis, is what we could scarcely expect. The insects in question fly little, save in the twilight. Much of their life is spent in underground burrows, or concealed underneath the cakes of dung upon which they feed. Hence there is little opportunity for either of their surfaces to be exposed to the action of the light. As, further, we find them passing

their adult existence often very early in the season, the invisible heat-rays can have here but a very doubtful influence. Besides we find kindred forms, of precisely similar habits, such as *Typhæus vulgaris*, black both above and below, without the least tinge of iridescence.

A still more unfavourable case seems to be that of the small Staphylinidæ, very dull and sombre in colour, yet always on the wing about the latter end of summer and the beginning of autumn, and well known in rural districts to the non-entomological public from their unpleasant habit of rushing into the eyes of passers-by, getting entangled under the eye-lids, and occasioning exquisite pain. These creatures encounter the rays of light to as great an extent, and in as many varied directions, as the Buprestidæ, yet there is no generation of gorgeous colours.

In thus criticising Mr. Lewis's views we speak with very considerable reservation, since his memoir had more the character of a preliminary tentative sketch than of a fully elaborated theory. But we must also remember the very serious objections which have been brought forward by Mr. A. R. Wallace, to the supposed influence of light as a main factor in organic colouration.

V. WHAT IS AN ORGANISM?

By R. M. N.

FROM day to day it becomes less easy to recognise and to justify distinctions which to our forefathers seem natural, broad, and striking. They felt no difficulty in drawing a hard and fast boundary between man and beast, between bird and reptile. To them animals and plants were creatures widely distinct, and incapable of being confounded together. But to them the animal was a lion, a horse, a camel, or, in the hardest case, a frog, a bee, or an earthworm, and such beings were not readily mistaken for a palm, an oak, a vine, or a grass-plant. But the more we have in successive ages learnt about animals and plants, little as it is, the more difficult do we find it to mark off one of these great "kingdoms" from the other. Even among scientific

specialists, obstinate controversies and much ink-shed may ensue before it is settled to which section some particular being should be referred.

Still more difficult is it to distinguish by any universal characteristics an organism from a portion of inorganic matter. The question is, of course, hardest to deal with when the organism is defunct, and we have before us merely its fossilised remains. It is not by any means decided whether *Eozoon Canadense* is a something which once lived and died, or a mere mineral structure. Sections of meteoric stones revealed what to not a few microscopists seemed organic forms, and many persons jumped at the conclusion that if dead organisms could thus reach our planet through the depths of space, so might the "moss-covered fragment of another world," which is credited with having been, for us, the beginning of life. The tyro in palæontology is often puzzled how to distinguish dendritic deposits of purely inorganic matter from the impressions of fossil ferns which they often closely mimic. The so-called Brownian or pedetic movements of certain powdery matters when thrown upon the surface of water have seemed to not a few persons evidences of life.

Nay, even when recent animals and plants, or the parts of such, are concerned, many well-educated people might be at a loss if asked: "How do you know that this mass of matter here before you is or was a living being and not a mineral product? Though they would perhaps rarely fail to make a correct diagnosis, they would as rarely succeed in rendering a right reason for the opinion they had expressed.

The first attempt at the definition of an organism is due to Aristoteles, and until recently it might be pronounced one of the most satisfactory on record. He discovered that in a plant or an animal, each part had certain duties or functions to perform; that it was, so to speak, a tool, an instrument or appliance (organon) for the totality to which it belongs. Every such totality, therefore, he named an "organism," that is, a combination of tools or implements. It is curious, as a contributor of the "Journal of Science" hints at,* how completely this original meaning of the terms organ, organism, and organic, has been obscured and lost sight of.

But on further research this definition of an organism was found to be at fault. Not to speak of certain parts in the higher animals which have become abortive and fulfil no function, we have become acquainted with certain low forms

* Journal of Science, 1882, p. 328.

of animal and vegetable life which are entirely homogeneous in their structure. Such beings would, by the definition of Aristoteles, be excluded from the world of life and classed as inorganic matter. Hence this definition, able and striking as it is, involves not the essence of the organic, but merely a prominent attribute of its higher forms. Dr. Roux also points out that this definition leaves out of account the activating principle, the "*encheiresin naturæ*" which holds this complex of implements together.

Self-consciousness has been suggested as the common characteristic of the organic world. Here, however, we encounter a twofold difficulty: we recognise self-consciousness in ourselves, and infer its presence by analogy in other animals. But we cannot demonstrate it to the satisfaction of dissidents in the lower animals and in plants. Some investigators even claim it as the exclusive prerogative of man. On the other hand, there are thinkers who maintain that even the inorganic world is not absolutely unconscious. It is easier to reject this supposition than to meet it with a formal disproof. Hence we cannot accept self-consciousness, or, indeed, any psychic function as the characteristic of the organic.

A mathematical distinction in structure has been suggested. It has been said that an organism and its parts are bounded by curved surfaces, whilst inorganic matter, when it becomes individualised, is included within plane surfaces, terminating in right lines, as we see in crystals,—the globular shape which liquids assume when in small portions,—being the "point of indifference" from which crystal and organism branch out in different directions. Here, however, we find in the lower forms of life, deviations which are fatal to the generality of this law. If we look at the magnified figure of the skeleton of *Xiphacantha*, as given by Sir Wyville Thomson, we find a structure presenting the lines and angles of a crystal. Nor do the different laws of symmetry, observed as prevailing respectively in organisms and in crystals, form the basis of a trustworthy definition. Five-sided symmetry is indeed unknown among crystals, but this fact is insufficient. It may further be said that a crystal if broken carefully is resolved into a number of smaller crystals of the same form, or at least into solids of some definite plane figure, whilst an organism if crushed or broken yields mere irregular fragments. Still, we must remember that certain polyps can be multiplied by section, each part becoming a distinct individual. Some authorities have attempted to find a characteristic of organisms in so-called

mechanical memory, that is, the permanence for a greater or less time of effects after their cause has ceased. But this is a universal property of matter in accordance with the law of inertia.

Organisms have also been defined as portions of matter individualised so as to be independent. This attribute, however, is common just as much to every process which is severed by its consistence, or by other especial attributes from its surroundings. Strictly speaking, independence exists nowhere.

Neither can we appeal to the transformation of energy, whether alone or in conjunction with the transformation of matter. This combined action is to be seen constantly in the weathering of rocks, and the evaporation of water.

The consistence of organisms is also not characteristic, though in their active portions it varies merely within certain limits, which may be a needful preliminary condition for the origin and maintenance of life. The percentage of water may range from 12 in certain seeds to 99 in some *Medusidæ*. It should here be remembered that we know of no organisms consisting entirely of liquids or gases without the presence of solids.

A certain chemical composition, common, with slight variation to all organisms, is probably an essential feature, but is yet not the essence of organisation itself, since the chemical constitution of animal and of vegetable protoplasm is probably not identical. These features being all set aside as not affording the wished for distinction, we come to some few considerations of another order.

The first really essential characteristic of organic beings is sensibility, which is common to all living beings, if not to all their parts. It may be considered, according to Roux, as the faculty of organic beings when exposed to the action of heat, light, or possibly of other phases of energy, to modify their form in a manner which cannot be regarded as a mere passive transformation, due to the external action, but is possible only by exciting a certain condition consisting in an intensification of cohesion, or in the removal of hindrances to its action.

Dr. Roux, however, very justly observes that we have here merely a special case of the general reactibility of all bodies. Still, although there are inorganic bodies, *e.g.*, mixtures of chlorine and hydrogen, which on exposure to light undergo a chemical change and become more coherent, being condensed so as to occupy less space, yet reflex movement is distinct to such a degree from all inorganic reactions that it may be regarded as a characteristic feature.

Taken by itself, however, it must be pronounced insufficient. If we imagine other well-known properties absent no one would pronounce any substance as organic merely from its being endowed with this attribute. Sensibility must therefore be regarded as a peculiar and very important subsidiary property.

Dr. Roux lays great weight on the relations of the organic processes to space and time. Under the former head is considered the spacial extension of organisms,—in other words, the kindred phenomena of growth and of reproduction. Growth, however, is no independent property, but indicates merely the quantitative relation of another faculty—assimilation—upon which it must be regarded as dependent. Growth, in the sense of mere enlargement, occurs also in the inorganic world. Thus, crystals may grow, and processes at first limited within a small territory may spread over larger region, as we witness in the formation of mists and clouds. The same may be said of propagation, which is merely growth beyond the limits of the individual, and is also dependent upon the faculty of assimilation.

More light is, in Dr. Roux's judgment, thrown upon the question by a consideration of the time relations of organic processes. These have, as far as we can decide, proceeded without interruption since their first origin. True, we find also inorganic processes which since their beginning have been likewise continuous, differing merely in extent and intensity. The rocks never cease from weathering, whilst the denudation of the land and the evaporation of the sea are unending. There is, however, an important distinction: the inorganic processes involving the transformation of matter continue so long as their outward conditions exist. Thus the weathering of a rock will continue for ever, as long as air, carbonic acid, and moisture are supplied. If it is removed from these influences the process of weathering to the best of our knowledge is arrested for ever, but on re-exposure, weathering is again and at once resumed. This is an important distinction between the organic processes and the various operations we see in play in the inorganic world. In the latter, the outward conditions are, as we thus see, all in all, whilst a vital process has in itself an internal cause of permanence to which the outward conditions are merely subsidiary.

A main attribute of every living being is the power of assimilation. The organic process can convert foreign matter into something homogeneous to itself, re-arrange the molecules of the bodies which it incorporates, and create its own requisites from what may be called raw

materials. One inorganic process, indeed,—to wit, flame, or rather, combustion—has the power of assimilating foreign matter, and, indeed, makes, in various important points, the nearest approach to the vital process, as we shall shortly see.

The power of assimilation may exist in very varied degrees, and upon them the permanence of such process will greatly depend. A process may assimilate less than it consumes, as in an ill-fed animal. Here the end cannot be far off. Or it may assimilate exactly as much as it consumes, in which case it can never extend, and is always liable to extinction on the least disturbance. Or, lastly, the assimilation may be in excess of all immediate demands. This is and must be the case in every permanent process. Hence, we find that along with assimilation the next universal requirement of organic beings is super-compensation of daily waste.

We find, too, in organisms, the power of self-regulation. The assimilation must depend on the degree of consumption. As the waste of tissue—the transformation of matter—increases, the introduction and assimilation of nutriment, in the widest sense of the word, must increase also. If increased work is not accompanied by increased hunger, or if nutriment is not attainable, or if when procured it cannot be assimilated, we have as the result the cessation of the process, or death.

But another essential point for the permanence of a vital process is the elimination of what has been consumed, *i.e.*, of the waste-products of the process. Were this process of elimination to go on at one uniform rate, then if the consumption were increased, there would be an accumulation of effete matter, which would react destructively upon the process itself.

It is curious to see how both these features of self-regulation are observed in combustion as well as in life. The more briskly the flame burns, *e.g.*, when urged by a current of air, the more abundant must be the supply of fuel, unless the combustion is to come to a speedy end. The more fuel is supplied the more ashes are accumulated, but under favourable circumstances the more readily they are dissipated by the increased currents of air.

Hence we see that when, in the language of poetry, flame has been always taken as a symbol of life; when we hear speak of it as "hungry," "devouring," &c., there is scarcely a departure from literal truth.

There are, however, certain distinctions between life and flame which Dr. Roux has not brought forward. Flame will go on for ever as long as supplied with appropriate

nourishment. Not so life, at least in any individual organism. No amount of food will enable any of the higher animals or plants to live on indefinitely.

Organic life is periodic or cyclical,—combustion not so. The organism has a fixed term of existence, and can grow and propagate itself only during certain terms of its existence, after which it dies away. Flame, if fuel is supplied, may not only last for ever, but can grow and propagate (the two processes being here one) during any part of its existence. But there is yet a more important distinction:—Suppose life extirpated on a planet, or on a single continent, all introduction of ova, seeds, germs, whatever being rigorously excluded, none of the forms of energy with which we are acquainted can call organisms, even of the humblest grade, into being. Life can only, within our experience, spring from antecedent life. With flame the case is quite otherwise. It can be kindled without antecedent flame, so long as the necessary materials are not wanting. Mechanical work can generate combustion, as when the savage kindles a fire by rubbing two sticks together. Chemical action can pass into flame, as when greasy cotton-waste sets fire to a work-shop. Electric action by a due arrangement can set fire to any suitable material, though no flame may exist within ten miles of the spot.

The fundamental attributes and the necessary conditions of life are self-regulation and super-compensation of waste. Where we find these, or where we trace arrangements for their production and marks of their presence, we recognise an organism or its remains.

VI. FORCE AND MATTER.

By S. Billing.

In the correspondence of the "Journal of Science," May, 1881, is a letter signed "A Lucretian." He asks, "Can any of your anti-monistic contributors reply to the following argument of Dr. Büchner's?—"It is evident that a force can only exist in as far as it is active; but a creative force, the first cause of the world, could therefore exist neither before nor after creation, since both before and after

this act it would have to be inactive, and inaction is irreconcileable with force." In your June issue H. M. C. remarks on the above, and his remarks I fully endorse. Dr. Büchner says "a force can only exist in so far as it is active:" this is quite right. Newton says of it, "*consistit in actione sola.*" But using force in its correct technical sense, it is clearly wrong to speak of the first cause of the world as a *force*. If we can give it any name derived from dynamics we should call it an *energy*. But an energy does not depend on activity for existence. The very title of Dr. Büchner's well-known book, "Matter and Force," taken in connection with its object, is enough to show he uses force (*Kraft*) in a vague unscientific manner, and that he is open to the complaint made by Clifford against numerous writers on the subject of force (*see* "Nature," June 10, 1880).

Lucretius's letter appearing shortly after my article, "The Formative Power in Nature," in your issue of February, 1881, and whilst an agreeing controversy was in progress between myself and your valuable correspondent, H. B., I could not but conclude that, although couched in general terms, the challenge was to either or both. It appears to me the arguments there used fully rebut Dr. Büchner's *assumptions*, for really they are no more. The Lucretian's query I determined to answer; the difficulty was the book. I supposed it to be published only in German, and therefore to me a sealed book. I kept the subject in mind, and lately found there was an English translation. These remarks I give as an apology for my long silence. The book is obtained and read, and now for my answer to its first chapter, which really contains the gist of the whole book—and a very insufficient book, when the momentous importance of the subject is considered, I deem it—and I can well join in the wonder its author expresses when he found his work was a success.

The edition I procured and from which I quote contains ten prefaces. Their perusal will show that they are not the work of a philosopher, and are wanting in that dignity of which the late Charles Darwin afforded so marked an example when assailed on every side because of his utterances, which really have reformed natural science. Unfortunately to surface, unappreciative, and unscientific men it is supposed to have afforded data to assail those who think that of NECESSITY there must be a God as the ordinator of Nature. The French revolutionists abolished God, but later workers in the same cause felt the *necessity* of a God, and decreed that if there were no God it would be necessary to

create one. Your readers are probably familiar with the answer of Napoleon to the *savans* who were with him in Egypt, when pressing on him their materialistic propaganda. He turned on them, pointing to the heavens, its grand galaxies being visible, and is reported to have said, "It is all very well, gentlemen, but who made all this!"

Before proceeding with my discussion a few remarks may be permitted. I am no bigot, and therefore ever willing to accept truth wherever it is to be found. I neither decry nor accept creeds, deeming the basis of one and all the same—*viz.*, the simple idea of an existing God. The only distinction I find in creeds is, that each endeavours to outdo the other in extravagant assumptions, to which and to the scientific ignorance of their propounders must be attributed the mischief so rife in the present day. How beautiful, acceptable, and logical is the simple conception of God as the creator and maintainer of natural phenomena. Had this idea been maintained in its primitive conception and taught in its integrity, how endearing and grand all social ties would have become. The idea of a universal brotherhood would have been realised with God as our Father. Humanity would then have become an intellectual being, but in lieu we have sense predominant with craft and greed as ruling attributes. I have no sympathy with theologies, but I have with the pure principles—which, alas! are merely ideal—they inculcate. I may be asked, What is God? It is sufficient to reply in the language of the Feduci when invoking the spirit: "I know not where Thou art, or what Thou art, but only that Thou art." It is impossible for man to define what is God: all that can be said is, that in phenomena we witness His manifestation, for therein we see intelligence embodied. If Professor Max Müller and those who worked in the same field have rightly construed their theories, if the cuneifom inscriptions and Egyptian hieroglyphics stand for anything, we must conceive man had a conception of spiritual things and a belief in an immortal principle or soul: the fact is intruded in the earliest historical records. Palæontology also brings its evidences. The Cro-magnon race contemporary with the great mammals had a belief in another life, M. Quatrefages ("Human Species, p. 328) says: —"The care bestowed upon burial places shows that the hunters of Mentone, as also those of Salutré and Cro-magnon, believed in the wants of their dead beyond the tomb. Our acquaintance with the customs of so many savage nations of the present epoch forbids any other interpretation of the interment of food, arms, and ornaments with the

body." It is a fair presumption that when man emerged from the animal *Homo sapiens* there was ingrafted in his intelligence (whatever its character) the religious sentiment. However crude it may have been, it is the power which has transformed our race and made it something more than animal. Speaking of Atheism, M. Quatrefages says:—"Obliged in my course of instruction to review all human races, I have sought Atheism in the lowest as well as in the highest, I have nowhere met with it except in individuals, or in more or less limited schools, such as those which existed in Europe during the last century, or which may still be seen at the present day" (*Ibid.*, p. 482). He further says:—"In these researches I have proceeded and formed my conclusions, not as a thinker, a believer, or as a philosopher, who are all more or less under the influence of an ideal which they accept or oppose, but exclusively *as a Naturalist*, whose chief aim is to seek for and state *facts*" (p. 483).

"Force and Matter" is the title of Dr. Büchner's work; had its title been Matter and Energy it would have been more in consonance with scientific ideas. Had the argument been conducted in a spirit of inquiry as to the existence of a formative power in nature and the confutation of a received idea, *i.e.*, the creation of the Universe by God, no one could have objected, for then the author would have stated his problem and enforced it with such scientific acumen, philosophical reasonings, and common-sense illustrations as he might be acquainted with. When he begins by saying, without adducing any arguments in support of his theorem, that those who talk of a "creative power" "are ignorant of the first and most simple principle found by experience and the contemplation of nature;" and in the same page (p. 80) he also says, "So the absurdities attending a belief in an individual creative power, &c." To quote the whole of such expletives scattered throughout the work would fill too large a space; those adduced must suffice for examples. They who are curious in such matters can consult the work, and I venture to say will be satisfied *ad nauseam*. Any person who really consulted the work with the desire to profit by the argument would by their continual occurrence not only be filled with disgust, the idea would also be present that the author, being wanting in reasoning power, endeavours to make up his deficiencies by invective and dogmatic assumptions. Even when passages of any force occur they are marred by the same fault. By the course he has pursued, Dr. Büchner must think that his assertions and assumptions are superior to all scientific, philosophical, and common-

place reasonings that can be adduced in contradiction. If he be convinced of his premises, it is quite allowable that he should endeavour to convert others to his views. An orderly presentment of his argument might attain such object, but the course he has pursued never yet convinced any one who really has the power of collating, weighing, and considering the arguments adduced. Doubtless, the course pursued may have had weight with incapable and semi-scientific persons, and also with those already possessed by Dr. Büchner's peculiar ideas; but with those who are conversant with the propositions and reasonings to be adduced on such subjects, the treatise would be in effect powerless.

The first chapter, "Force and Matter," in which the potency of force and matter in creation is asserted and the impossibility of there being a creative power, contains the pith of the whole work. But it is well to bear in mind the teachings of Professor Huxley in his lecture "On Descartes's Discourse on Method." He says: "I am prepared to go with the Materialist wherever the true path of Descartes may lead. . . . But when Materialists stray beyond the borders of their path and begin to talk of there being nothing else in the universe but matter and force and necessary laws, I decline to follow them. . . . Matter and force, so far as we know, are mere names for certain forms of consciousness."

"No force without matter—no matter without force, neither can be thought of *per se*. Separated they become empty abstractions" (p. 78). In other words, force and matter are so intimately entwined that without the one the other could not be. The author adopts Mulder's observation, "that forces cannot be communicated but merely called into action." From this it would seem to follow that the forces are inherent in mattter, and it would then seem that force and matter are synonymous terms. It is possible that in the origin, so far as matter is concerned, there was but one condition, matter or fluid, and that out of it all phenomenal combinations have resulted. The conclusion that Dr. Büchner appears to draw is, that the correlate forces "are but changes in the aggregate state of matter." "Light and heat are vibrating, undulating bodies." "Electric and magnetic phenomena (Czolbe) arise . . . like light and heat from the reciprocal relations of molecules and atoms." Molecules and atoms, Professor Huxley has said, are but imaginative symbols, and that he who mistook them for real quantities would err equally with the mathematician who should so mistake his *x*'s and *y*'s. At all events, no one has ever seen

the one or the other. In a work so exacting as that in question no words should be used expressing forces or substances that are not the tangible or visible results of observation and experiment. We know that matter is divisible into the minutest particles, but of these particles there is no proof that they are either atoms or molecules. A substance is pounded into the merest dust, and can be further disintegrated so as to become undiscernible by the minutest microscopical tests. Professor Crookes has shown that matter can be resolved into a fourth state and a beyond—darkness. Professor Tyndall has shown that there is light only when there is a substance to reflect it. Here there appears to be neither substance nor force; what then becomes of this indestructible matter, on which our author so pertinaciously relies? "Nothing but the changes we *perceive* in matter could ever give us any notion as to the existence of a power which we qualify by the name of force" (p. 79). By Mr. Crookes's experiment matter has been pursued into an ultimate range wherein neither matter nor force could be perceived. And further we must remember that, however identical matter and force appear to be, the static state never becomes the dynamic without an impulse; the impulse is foreign to or outside that in which the action occurs. Surely there must be a something (whatever that something be) besides force and matter. It follows, as a necessary consequence, that were there no external impulse, force and matter would have for ever remained enwrapped in the embrace of the one and the other, and this glorious world, which we know as a world of action, would have been an utter void phenomenon, and its realities would never have been.

Truly we are cognisant of the action of force only by means of the senses; but when the question is of power, we have quite another subject to consider. There are mental powers which act from within and create, *e.g.*, pleasures and delights, distress and miseries. These powers are wholly without the scope of the senses, however they may react on them.

Because we know the action of force by means of the senses the author jumps at the conclusion that the world cannot have been created. He continues, assuming a creative power, soul, &c.:—"Time could not have existed before or after creation; it could not have existed before because the notion of power is not reconcileable with the idea of nothing or inactivity." "It could not have existed after the creation, as rest and inactivity are again in-

compatible with the notion of force." A steam-engine has always its force, although not always manifested. So a man may be intensely intelligent, yet not always exerting his powers. Watt employed the reflection of years before he presented his thought to the world enbodied as a steam-engine; but by the act his intellect was not impaired. Time, as we understand it, is an invention of man to suit the circumstances under which he exists, and is really but the way mark for a succession of events. "A force which is not manifested does not exist." Does the author understand the meaning of the static and dynamic states of force? One would think not, because he says that a force not manifested "cannot be taken into account in our reasoning." He then says, "there remains but a third possibility . . . that creative power has suddenly and without any occasion arisen out of nothing—had created the world (out of what?), and had again in the moment of completion collapsed within itself, and, so to say, dissolved itself into the universe." Intelligence may be used and by use become more powerful; repose, to awake again in its pristine power; and so it may be that when the creative power embodied its intelligence and presented a world with all its necessary energies, the intelligence relapsed within itself, again to be used, and so on to all eternity, but never spent by action.

Helmholtz says that "there is no mechanism but that which is the result of intelligence," and if we go to our world life, we find no invention that is not an embodied thought. We see the machine, we also see in it the embodiment of the intelligence of its inventor. We look on a grand picture. What do we admire, the paints or their arrangement? In their disposition do we not see the mind and skill of the painter staring at us from his canvas? We do not suppose the glorious conception thereon embodied was the accidental arrangement of the pigments. The painter's mind is interfused into his work. He embodied the ideal his mind presented, and it is that mind which excites its sympathetic action in the mind of the beholder.

This argument being extended makes it possible (assuming that there is a creative power notwithstanding Dr. Büchner's assertion that there is not) that the creative power having completed the nucleus, out of which the glorious panoply we term nature arose, by an interfusion within this nucleus of the intellect and energy which conceived it, and then became sufficient for its own development and maintenance; hence this energy, which is the inherent power of phenomena, is always active. This is a simple conception of creative power

—of God if you will—but in it there can be no idea of His "reposing after creation," but yet as an individual capable of again suspending his own laws."

The idea of a god, other than those of the vulgar, was entertained by Plato, Socrates, Epicurus, Aristotle, and others of the Greek philosophers. The abundance of the myths of ancient Greece shows the religious reverence the people had for their gods. Their superstitions being excited, the gods were personified and pictured as caretakers, and were supposed to be present in the minutest relations. They were, in fact, the presiding providence of the people (*vide* "Grote's Greece," vols. i. and ii.) If Dr. Büchner had not been engrossed by preconceived views, his comments would have been directed to that conception of God which engrosses the minds of philosophers rather than to those phases of His being delineated by ecclesiastics. Mr. Herbert Spencer ("First Principles," p. 37) says:—"We cannot think at all about the impressions which the external world produces upon us without thinking of them as caused, and we cannot carry out an inquiry concerning their causation without inevitably committing ourselves to the hypothesis of a first cause." And when Dr. Büchner says the unknown is to be interpreted by the known, the question at issue appears to be decided. Chemistry and mechanics, as we know them, are all developed by the intelligence of man; and so, by a parity of reasoning, it is a fair conclusion that the chemistry and mechanics of nature were the result of intelligent direction. It appears idle to imagine that the framework of the universe, or even the mechanism of an animal, could have arisen from casual adaptations. There is a grand series of formations extending from the protoamœba up to man. How could their orderly arrangement have arisen excepting by an internal energy engrafted by an intellectual power into the germ, endowing it with the capability, by slow gradations, by differentiation, adaptation, and development to attain that perfect fitness which is found in all organisms. We have something more here than force and matter and necessity. All forms of life are a pushing from a vital centre. Aristotle held that the universe originated from one unmoving centre.

From all we glean from nature, we are bound to conclude that the purpose and plan of objective phenomena was conceived once for all, and that there were no tentative efforts and corrections. The mystery is not so much the differentiation we everywhere observe as the principle or power by which they are produced. The conception of the plan of the evolution of forms—the homogeneity everywhere observable

shows its factor was an intelligence, or we should not have gradations of development, as proved by the successive phases of embryonic changes, for it would have been as easy to have presented the perfected forms.

What then is the architect of nature, God or accident? The former alternative has been considered. Accident, not one but an interminable succession, and so opportunely occurring that when the necessity arises an accident is ready to meet it, and these successions are so endless and of so precise a character that they create uniformity and order, adapt parts to parts, and each to the whole, not one whole but many millions of wholes for each organic form, whether animate or inanimate, due to a fortuitous concurrence of chances. If Dr. Büchner would apply his mathematics, for that is the scientific test now-a-days, and lived a life as long as the world has had being and had space for his numerals, he possibly might find the sum of the chances by which all these accidents would coalesce and produce unity. Such assumptions—they are not reasoned hypotheses—have no basis for argument. All will agree with Liebig "that no force can originate from nothing," and with Czolbe, "an absolute nothing is not cogitable." What, then, is the *something* which sets matter in motion? Dr. Büchner does not tell us whence, where, and what was the originating impulse. Were the universe alone possessed by matter and force, we should have but inertia, no grind of worlds, no animate forms, a universe of nothingness. Day by day we see the human intellect creating, yet intelligence is not matter, although it uses matter in its embodiment when it becomes phenomenal. What, then, is this something but intellectual energy which disintegrated the chaos (the voluntary motion of Lucretius?), assorted its elements, and which condensed and gave to them orderly assimilations. The true and common sense solution of all cosmic problems is, the originating cause, be it what it may, produced an effect, and this effect produced another effect, and so became the cause of the latter; so every effect becomes the cause of the succeeding effect, and so on to the end of the chapter. Thus we have a continuing beginning—an increasing energy instituting an untiring action—all due to the originating impulse, which of *necessity* is independent of the force and matter on which it acts.

If we could adopt necessity (*causæ efficientes*) as the acting impulse we still have a something alien to the thing acted on, and we should also remember that with our ideas as to natural results there are many things in nature which we

deem unnecessary, *e.g.*, diseases, parasites, &c. Necessity would cease when the needed forms were produced, inorganic and organic, and such phenomena as are necessary for their support and maintenance. No creature would require more than an instinctive power for generation, preservation, and adaptation to climatic changes. If, then, all that is animate is to become inanimate, there would be but a succession of changes; and man, what place could he have in this scheme of necessity?

Can we conceive that the purpose of creation is accomplished by man being born but to die? We can conceive that the wondrous development of organised forms was instituted that man might be; but if there be after this life no beyond for intellectual man, he exists but as a thought, and expires in its utterance; and all we know of that immensity, the universe, is as of an ingenious and wasted mechanism springing from nothing and ending in nothing. Intellect in such a corollary would be no necessity, but whence this necessity? Did it create itself and its own impulses? If so, we do not seem far short of an intelligent creator; but then this necessity is governed by law—whence came these laws? Accidental recurrences! Law implies not only a law given but a law maintainer; hence intelligence direction. If this be the outset of *causæ efficientes*, do we appear to be very distant from the *causæ finales?* Experience shows there is no finality in the finite. Each man in the impressiveness of his own consciousness must judge in his own conceptions—by faith or by facts, a God and intelligence, no God but matter, a creator or a chance. God the universe and man as a collective fact (whatever be the postulates of Faith) is an open secret only to be resolved in the Unknown.

POSTSCRIPT.—Dr. Büchner's work, "Force and Matter," was read by me with the intention of comment and answer; but I find the intended task was completed even before I had heard of Dr. Büchner (if I except an isolated observation occurring in Dr. Elam's "Winds of Doctrine"). My observations are mostly confined to the first chapter of the work—viz., Force and Matter, with its incidents, necessity and the no God hypothesis. If Lucretius is unsatisfied and requires further answer, I refer him to my work, "Scientific Materialism," where he will find all the hypotheses of Dr. Büchner anticipated, argued, and, as I conceive, refuted.

VII. HARVEST SAVING: THE READING COMPETITION.

IT might reasonably be expected that the Royal Agricultural Society of Great Britain would be the very foremost body in the kingdom to promote means for arresting the severe loss which we seem destined to undergo from bad weather at harvest time,—a loss estimated at the average figure of six millions sterling yearly. But what must we think when we find this powerful and influential organisation, not merely looking on in apparent apathy, raising no finger to put an end to this national calamity, but as far as in it lies seeking to discredit the efforts of private individuals?

The results of the Reading trial are, we must say, exciting both surprise and indignation, and many persons are beginning to ask what is the *raison d'être* of an Agricultural Society which can thus mistake its duties? The facts are simply as follows:—Mr. Sutton, the eminent seedsman, of Reading, doing what the Society has strangely omitted to do, offered a prize for the best mode of drying hay artificially, and the Society then appointed three judges to decide. Mr. Gibbs, whose drying machine we have noticed on two former occasions, rightly considering that its efficiency had been already proved in practical working beyond all reasonable question, declined to compete. A gentleman, however, who had bought one of these machines and used it with complete success on his own farm, sent it in for trial. Only one other apparatus on a quite different principle appeared on the field. The material to be dried consisted of coarse marsh-grass, which had been shaken out by hay-tedders whilst the rain was falling, exposed on the ground to five day's rain, so as to get as wet and dirty as it could. As a morning paper aptly says: "It was trampled and kicked about by enraged visitors, and rolled in by nearly all the dirty little boys and girls of Reading." Being in this condition, the Gibbs machine dried it in six hours, so as to be fit for stacking. It has since been sold by public auction at the rate of £3 6s. per acre. The grass dried by the other process, after being under treatment for a much longer time, only fetched one guinea per acre.

Of course all right-thinking persons will conclude that

upon the facts before them the judges could have no other option but to award the prize to the Gibbs machine. But no: a man "convinced against his will" can generally find some loop-hope for escape. The judges therefore refused to award the prize at all on the singular ground that this system did not make such good hay as could be produced by field-work! Were this assertion as true as it is inaccurate it would still be no justifiable plea for withholding the prize. The farmers' difficulty comes from the fact that in many seasons field-work is useless. There are persons who question, not only the fairness and the logic, but even the legality of the decision. Suppose a prize offered for the best make of artificial teeth: could the judges refuse an award on the ground (true or false, as the case might be) that the best were, after all, inferior to natural teeth?

We could with very little trouble put our finger upon a man, who, at this same Reading trial, overheard a gentleman connected with the Royal Agricultural Society exclaim: "Oh, this is the scheme I threw cold water on at Manchester, twelve years ago!" We hope this gentleman feels that he has not lived in vain. He has done what in him lies to prevent a useful invention from being adopted, and to perpetuate a heavy national loss. It is true the agricultural papers with one accord admit the value of the Gibbs machine; it is true that the testimony of such gentlemen as Mr. G. P. Fuller, of Neston Park, Corsham; of Dr. Tristram, Q.C.; of Mr. Botting, manager of the Duke of Northumberland's home farm; of Mr. C. J. Morris, J.P., of Wood Eaton Manor, Stafford, &c., ought to outweigh the results of this unfair award at Reading. But, unfortunately, very little is needed to encourage the British farmer in his almost fatalistic apathy under removable evils, and his reluctance to accept the means of safety when offered. How we, with our method of treating inventors, ever became a great industrial nation is a miracle.

VIII. SAFETY CHEQUES.

IT not unfrequently happens that when an inventor has devoted years to perfecting some idea and bringing it into a practical, business-like shape, and finally secures his results by means of letters patent, others appear upon the scene and offer us something aiming at the same object.

Thus, once upon a time, all the world was inventing new methods of obtaining red colouring-matters from aniline oil. Another time all the world was no less busy devising new forms of roller skates. More recently all the world is up to its dazzled eyes in electric appliances, seeking to produce lights, whether "incandescent" or "arc," cheaper and more manageable than anything which has gone before. Hence, it appears that there is a class of inventors who seem always ready to turn their minds to any subject which attracts public attention. Among such less sensational, but still very important, subjects, we may mention methods of preventing a very dangerous fraud of the day,—the alteration of cheques after they have been filled in and signed by the drawer.

This problem appears to have been solved in the most satisfactory manner ("Journal of Science," 1881, p. 104) by Mr. A. Anthony Nesbit, F.C.S. His cheques have been pronounced by chemists profoundly versed in the properties of colouring-matters to be absolutely incapable of fraudulent alteration. So that on this score no improvement is needed or is even possible. But, of course, if any future inventor can give us *equal* security at a *less* cost for materials and labour, and with *less* trouble, he may claim the superiority. Has this been done, or is it likely to be done? The Nesbit process requires no specially prepared paper; any ordinary quality used for cheques will suffice. No complicated engraving is needed: the cheque is merely tinted with one of the colours, which are changed in different ways respectively by acids and by alkalies, and is then printed, a part of the design with an acid and another part with an alkaline ink. Simplicity, despatch, and economy in materials and labour can scarcely be carried further.

One of the two attempts which have since been made to supersede the Nesbit cheque is not proof against alteration, as we have seen experimentally proved. A third process has been devised and patented by Mr. Nowlan, and is chiefly remarkable for its great complexity. As far as we understand the specification and claim—the latter not remarkable for clearness—Mr. Nowlan's cheque is to consist of two or more layers of paper, as thin and transparent as possible. These leaves are printed with certain colouring-matters, some of them alterable and others permanent, and are then cemented together with their printed sides innermost. The designs, lines, letters, &c., will then have to be read *through* the uppermost paper, upon which, of course, the drawer's signature and the sum will be written.

We have never seen any experiments performed upon a

cheque of this kind, and have therefore no right to say that it admits of being tampered with. But, supposing that, like Mr. Nesbit's, it is absolutely secure—and it cannot be more so—how do the two compare for convenience and simplicity? It is obvious that each layer of Nowlan's cheque will require at least as much time, trouble, and expense in printing as an entire cheque on the Nesbit system, and after all comes the cementing together! A word as to this same cementing process: We find Mr. Nowlan saying that the cement used must, on the application of moisture (*i.e.*, for the attempted separation of the two sheets), induce spreading or blurring of the pigments, &c., and thus spoil the cheque. But if this is the case, what will happen when the wet cement is first applied to the sheets in order to fix them together? Will not blurring or spreading at once take place?

Reserving, however, all questions of this kind, we contend that any man capable of understanding the facts of the case will at once assign the palm of merit to Mr. Nesbit's process as compared with any other as yet known.

ANALYSES OF BOOKS.

Proceedings of the Society for Psychical Research. Vol. I., Part 1. London: Trübner and Co.

THE "Society for Psychical Research" organised at the beginning of the present year is no mere demonstration. To work it has gone with a good will, and in the pamphlet before us it gives the first-fruits of its activity on a much-contested subject. "Thought-reading" has, thanks to the public exhibitions of Mr. Bishop and Mr. "Stuart Cumberland," attracted no small portion of attention. Journalists—scientific, unscientific, and even anti-scientific—have opened their columns to records of incidents which forty years ago would have been curtly rejected. Without an unpardonable digression we may here remark that there is at the present day a decidedly increasing relish for the so-called supernatural. What this change in the public taste signifies it would be rash to predict. A sceptic has, indeed, ventured the very just remark that all our social intercourse is based upon the assumed impossibility of our thoughts being read otherwise than through language, signs, gestures, changes of countenance, and the like. Yet at the same time certain of the experiments performed, if correctly reported, seem to indicate that the thoughts of any person may, under certain circumstances, become known to others without any of the usual means of communication. It is natural and right that all such experiments should be scrutinised with jealous care, and that every opening for fraud, collusion, and self-deceit should be eliminated. To do this has been the especial task of the Special Committee on Thought-reading.

This Committee, in a Report given in to the Society July 17th of the present year, states that "The present state of scientific opinion throughout the world is not only hostile to any belief in the possibility of transmitting a single mental concept except through the ordinary channels of sensation, but generally speaking it is hostile to any inquiry upon the matter." We cannot venture to impugn this assertion, but we must regret that the latter clause should be true. Science, we think, should not refuse inquiry except in cases where there is a *prima facie* absurdity in the way. We may perhaps here usefully attempt to settle when an inquiry may be safely and justly refused as waste of time, and when it should be undertaken. Suppose a man who has scarcely mastered the elements of arithmetic, and who is totally ignorant of the higher mathematics, comes forward to point out errors in the astronomical calculations of Leverrier; suppose another, in

the teeth of a vast and ever-increasing mass of evidence, tells us that the earth is flat, and that phosphorus is soluble in water as such; suppose that a third, without any systematic knowledge of biology, without any original observations or experiments, undertakes to overturn what is commonly called "Darwinism," we are not bound to undertake any formal refutation of their errors. Before they could understand our arguments they would be compelled to acquire knowledge which they at present do not possess. But, on the other hand, if a man habitually truthful, sober-minded, and having no personal interest in the matter, lays before us some novel fact which he has observed, we have no right to refuse him a hearing because no such facts have been registered before. To take a very simple instance: it has been held, on the foundation of general experience, that none of the many species of lizards known to Science were capable of inflicting a venomous bite. Latterly, however, a species of lizard has been discovered in Mexico which possesses true grooved teeth and poison-glands, and whose bite proves deadly to small animals within a few minutes. Hence the inductive conclusion that "no animal having legs can instil poison by biting" has been set aside as founded on too narrow a basis. In like manner a wider experience may compel us to reconsider other inductions, and hence we should never dismiss the records of facts on the mere ground of their unexpected nature.

In the Report before us an abstract is given of Dr. Beard's summary of the sources of error in experimenting upon living human subjects.* But the writers think that he has omitted one source of error more fatal than any other to a right interpretation of the facts, namely, a strong prejudice against the possibility of the phenomena in question. They admit, however, that before a promiscuous company experiments of this kind are nearly sure to be vitiated by some of the sources of error which Dr. Beard has pointed out. These objections scarcely apply to the observations made by Prof. Barrett upon the Creery family. There was on these occasions often no person present who could, even if so disposed, have given any hint or indication as to the word or object thought of. Often, too, no one save the experimentalist knew the name, the word, or the object which he was thinking of, and which one of the daughters of the Rev. Mr. Creery, or his maid-servant, succeeded in guessing—if the word may be used. The failures, we think, will prove quite as instructive as the successes. Prof. Barrett writes that the first test was to name the hiding-place of some small object chosen by himself. The children succeded in only one case out of four. The next demand was to give the name of some familiar object agreed on in the child's absence. Here there were six successes out of fourteen cases. Perhaps the hardest trial was to think of

* *See* Journal of Science for July, 1881.

imaginary names, which the children were to guess; they succeeded in five cases out of ten. Three cases were complete failures; but in the two remaining the names given had a resemblance to those which had been fixed on. "Jacob Williams" was given as "Jacob Wild," and "Emily Walker" as "'Enry Walker," the guesser in the latter case being the maid-servant.

In the second set of experiments, performed with the same children, by Messrs. Myers and Gurney and two ladies who were entire strangers to the family, we remark that of three fictitious names to be guessed all were failures, but that in two of these cases the names guessed were somewhat similar to those which had been selected by the experimentalists. For "Martha Billings" they guessed "Martha Biggis," and for "Catherine Smith" "Catherine Shaw." In guessing a card selected at random out of a pack they were successful in nine cases out of fourteen. On two other occasions there were eight consecutive successes, once with cards and once with names. These results practically exclude coincidence, since in the former case "the adverse odds were over 142 millions to 1, and in the latter incalculably greater."

In other cases "Chester" was guessed for Leicester, "Freemore" for Frogmore, "Singrore" and "Grover" for Snelgrove. It is conceivable that by collating those cases where the word guessed approximates to that selected some light may be thrown on the method in which thought is communicated from one brain to another. We hope that the Committee will therefore continue their interesting observations, though we scarcely see that the evidence thus obtained can be decisive as against the materialistic philosophy.

The Victorian Review, August 1st, 1882. Melbourne: The Victorian Review Publishing Company, Limited.

THIS number may be noticed as containing one of the most able and thoughtful of the many articles which have been called forth by the death of Charles Darwin.

The author, T. Jeffrey Parker, fully appreciates the man whose eulogium he delivers at the Antipodes. He declares that "in an age of eminent naturalists it occurs to very few to question the right of Darwin to a position above them all as the father of modern biology." He then, as a key to the position, gives a brief sketch of the history of philosophical biology as distinguished from the mere collection of desultory facts. He notices the labours of Wolff, Linnæus, the elder Darwin, and Buffon. He considers that the last of these worthies "would in all proba-

bility have given the world a far more consistent theory of Evolution than that we now find scattered up and down the numerous volumes of his 'Histoire Naturelle,' had it not been for the blighting influence of the Sorbonne." He adds in a note that on first reading Mr. Butler's "Evolution Old and New" he was "greatly taken with the theory that the passages in the earlier volumes of the 'Histoire Naturelle' which avow a belief in immortality are ironical. But on reading Buffon himself I find the theory rather more difficult of acceptance."

Among the naturalists of the beginning of the present century Mr. Parker notices Goethe, Treviranus, and Saint-Hilaire, merely in a footnote, and overlooks Oken altogether. Of Cuvier he takes, in our opinion, a too favourable view. He describes that great type of the official bureaucratic *savant* as "devoting himself to facts." True, always provided that such facts did not militate against his preconceived theories, such as the immutability of species, the recent origin of man, the series of geological catastrophes, and the like. But we have heard of certain fossil human remains which he declined to examine, and which were ultimately mislaid. Preserve us from such devotion to facts!

The author, however, in maintaining that, from the disappearance of Lamarck to the publication of the "Origin of Species," philosophical biology was completely in abeyance, overlooks the so-called "Representative System" of Macleay, Swainson, Vigors, and their adherents. This system, sometimes known as Quinarianism, collapsed not on contact with Evolutionism, but by its own internal feebleness. Yet at one time it completely dominated British natural history. It tinctured the writings of Kirby and Spence, of Edward Newman, Stephens, of Professor Westwood, as well as the biological portions of "Lardner's Cabinet Cyclopædia," of "Knight's Penny Cyclopædia," and of other standard works. Nor without at all overlooking its many extravagances can we say that it does not contain the germs of certain truths which it might not be labour lost to extract and to purify.

To return: full justice is done by Mr. Parker to the singular philosophic caution of Darwin in refraining from publishing his views until he had fully weighed the evidence upon which they rest. This point has a further bearing. Our great leader has been accused of a reticence bordering upon dishonesty for not at once extending his theory to include the human race. But the man who after five years' work merely allowed himself to speculate, and who, not until the end of two more, merely drew up some short notes, not yet for publication, but merely for the criticism of one or two friends, was simply true to Science and to himself in allowing twelve years to intervene between the appearance of the "Origin of Species" and that of the "Descent of Man."

Mr. Parker gives a somewhat humorous sketch of the great

Darwinian controversy which, he considers, is approaching its final stage :—"The opinion has already been advanced that St. Augustin, St. Thomas Aquinas, and Father Suarez were Evolutionists, and I should not be in the least surprised if before long the same discovery were made with regard to Knox, Calvin, and Wesley. Certainly a very good case could be made out for Dr. Watts; a line in one of his hymns—'From change to change the creatures run'—is simply Evolution in a nutshell."

But let us not exult prematurely. Ignorance has learnt the art of organisation, and evil days may yet be in store not for Evolutionism alone, but for Biology in general, and for Science altogether.

The Solution of the Pyramid Problem, or Pyramid Discoveries, with a New Theory as to their Ancient Use. By ROBERT BALLARD, M.I.C.E. New York: Wiley and Sons.

INDEPENDENTLY of the recent Egyptian troubles, the pyramids have of late years attracted a great and increasing amount of attention. Most of our readers will be more or less acquainted with the views of Mr. Piazzi Smyth, the Abbé Moigno, and others, who look upon the Great Pyramid as a divinely inspired record, embodying profound astronomical, mathematical, and meteorological truths.

Mr. Ballard considers that the builders of the pyramids were by no means devoid of mathematical knowledge. These structures he considers were not erected aimlessly, but to serve a very important practical purpose. In Egypt the annual overflow of the Nile is apt to render the boundaries of lands doubtful: hence rapid and efficient means of surveying were needed. These means were furnished by the pyramids, the respective positions of which are important. The Cheops stands at the acute angle of a right-angled triangle, of which base, perpendicular, and hypotenuse are to each other as 3, 4, and 5. Mycerinus is at the greater angle of this triangle. Cheops is situate at the acute angle of a right-angled triangle, of which base, perpendicular, and hypotenuse are to each other as 20, 21, and 29. He considers the Royal Babylonian cubit (= 1·685 British feet), which seems to have been used by the builders of the Pyramids, as the most perfect ancient measure yet discovered: 77,760,000 of these cubits are equal to the polar circumference of the earth.

Our author does not, like Mr. Piazzi Smyth, find any striking difference between the pyramid of Cheops and the rest. "That one particular pyramid should have anything to do with the past or the future of the lost ten tribes of Israel" seems to him "the

wildest conclusion of all, except perhaps the theory that this one pyramid points to the future of the British race." He recommends that the pyramids should be re-cased, when their value for surveying will at once appear. At the same time he admits that they may have had subsidiary uses, and recommends borings near and within them for the purpose of detecting subterranean vaults.

The author of this interesting work may be communicated with at Malvern.

The Field Naturalist and Scientific Student. October, 1882.

In this issue we find some very interesting matter. Dr. R. Angus Smith, who rarely touches upon any subject without saying something worthy of note, gives a paper on National Voices. The peculiarity of the Scottish voice has, it appears, previously been remarked. Now Dr. R. A. Smith, when in Germany, heard an engineer explaining an apparatus for cutting and preparing peat, and was struck with his voice, which had a distinctly Scottish character. The speaker was, it appeared, not a true German, but a native of Slesvic, from which region many of the so-called Saxon immigrants to Britain must have come. In spite of the lapse of time and of many changes the vocal character has thus clung to the race on both sides the sea. Such similarities, Dr. R. A. Smith suggests, may be useful in tracing out the affinities and the wanderings of nations.

A note by Mr. H. Kerr raises the question as to the introduction of the wild cherry into England and Scotland. It may be remarked that in some parts of the North of England this species is known as the "merrie" tree,—a synonym pointing to its French name *mérise*, and perhaps an additional piece of evidence in favour of its having been introduced into Britain by way of France.

A lady who uses the signature Rosa pleads for the "popular names of birds and plants as each contributing a little information." But what if the information thus contributed is incorrect?

Dr. R. A. Prior communicates a paper on "Errors in the Popular Names of Plants." Here he informs us that for more than 300 years the name "Forget-me-not" was given to *Ajuga Chamæpitys*, whilst our Forget-me-not then went by the clumsy name of "Mouse-ear Scorpion-grass." The change of name is said to have taken place as recently as 1821. We must, however, remark that the new nomenclature with the legend of the drowning lover took root very rapidly. In 1835 we were shown the precise spot in the Rhine where the gallant knight had

perished in the treacherous embrace of the water-weeds, and the story was represented to us not as a modern invention, but as a veritable old Volksmärchen.

The question "Do fishes feel pain?" is answered in the affirmative by W. J. V. Vandenbergh. Mr. Cuthbert Oxendale gives an instance of a cat hooking fishes out of a mill-lodge with her claws, and is of opinion that if the taste for fish had been old in cats they would ere this have taken to swimming.

Mr. Leo H. Grindon, in an article on "Poisonous Plants and Harmless Ones," ventures on the bold statement that "we may be sure that nothing in Nature has been created without a purpose benevolent in the end."

Our Medicine Men: A Few Hints. By H. STRICKLAND CONSTABLE. Kingston-upon-Hull: Leng and Co.

Fashions of the Day in Medicine and Science: A Few more Hints. By H. STRICKLAND CONSTABLE. Kingston-upon-Hull: Leng and Co.

WE have in these two volumes a mixture of matter, wise and absurd, original and thread-bare, grave and gay, to the point and very much beside it, of anecdotes and quotations, all embedded in a gangue of anti-vaccinationism. Some years ago, we are told, a child of the author's nearly died from vaccination. "Hence this book."

To find a man who could either approve or disapprove of all that is here written would be impossible, and to expound and discuss all the various views here brought forward would occupy at least a yearly volume of the "Journal of Science." Therefore, after pointing out that, according to the Introduction, by "Our Medicine Men" the author means "our professors of the art of healing, our professors of physical science, and our professors of religion," we will proceed to lay before our readers a fair selection of the views here set forth.

In Appendix A, as well as in a number of scattered passages, we find A. Comte dealt with in a well-deserved and therefore not laudatory manner. He rightly reminds us that, according to the best authorities in every department, Comte was, scientifically considered, simply nothing. He conducted no researches, he made no discoveries, and was even not well versed in what had been done by others. He had some appearance of being a mathematician, but Arago tells us that even in that discipline he is not worth notice. And if we test his merits in other directions we find him upholding the dualistic chemistry of Lavoisier

when already tottering to its fall, and closing his eyes in biology to the dawn of Evolutionism. We may well ask, Can this man, so devoid of philosophic insight, be the imagined law-giver and prophet of Science—the man who was to do for the nineteenth century more than Bacon had done for the seventeenth—but who, instead, has merely founded a new sect?

On one point Mr. Constable is in error. He says: "Most Comtists believe in Darwin's theories." Not so; like Comte himself, Robin, his successor in the pseudo-papacy, and their followers are adherents of the Old Natural History, G. H. Lewes being perhaps the only exception of moment. Positivism, contrary to what a hasty reader of the "Philosophie Positive" might infer, is not merely unscientific but anti-scientific in its tendencies. It formally boasts of setting limits to the "inroads of Science," and should it ever possess the power, it has the will to take up the *rôle* of her enemy and persecutor. We can forgive Mr. Constable a multitude of sins for his hearty, honest loathing of Positivism and its cant.

John Stuart Mill is also somewhat severely dealt with. He is described as a "sort of a decorous Voltaire, but without the charming wit of the latter." But we think that the author errs in taking either Comte or Mill as types of men of science. They were no such thing. They were essentially readers, men of books and paper, not observers and experimentalists—men of things. It is a great mistake to assume, as does Mr. Constable, that even the "third rate man of science"—an expression repeated more frequently in these volumes than we have time to count—is rendered torpid by analysis. Whenever they meet with moderately fair play, that is, when they are neither burnt at the stake, imprisoned, nor caused to perish for want of the common necessaries of life, they rank among the happiest members of society. In some minds the hunger and thirst for knowledge, for truth, is a consuming passion, quite independently of any advantage which may accrue to themselves from their discoveries or inventions. Only those who have felt this passion can judge of the intense gratification which scientific discovery affords.

What must we say to this passage?—"We often see that the man who works with unceasing brain-work, and does nothing else, though he may learn to perform marvellous gymnastic feats with his intellect, pays a fearfully heavy price. Gradually, as the years pass, he freezes and hardens. Natural affections vanish. His soul, if ever he had one, flies away, and he is left, a piece of wonderful machinery, warm as ice and soft as flint. Still he sometimes does work that the world wants." Here, again, we have proof that Mr. Constable does not know the man of science. Let him look at Darwin, engaged in research and discovery from his youth up to within a few hours of his death. Yet everyone knows that he was kindly, gentle, beloved, and happy in his

family circle, kind to the poor, modest, and humble. The author's description may be true of the men who are "reading" for degrees, honours, &c., but it is not true of the men who are "working" for hitherto undiscovered truths.

Concerning spiritualism the author says: "The unexplained facts that come under the heads of somnambulism, hysteria, mesmerism, table rapping, &c., which remain after much imposture is allowed for, cannot be denied by any but the straitest sect of scientific pharisees. But to attribute them to anything but abnormal workings of the mind, or to anything that people think they mean by the term 'spirits,' is, in the present state of knowledge on the subject, simply superstitious." Here, then, we see Mr. Constable firing off both barrels, and hitting Dr. Carpenter on the one hand, and Mr. A. R. Wallace and Prof. Zoellner on the other. But there is some truth in the following passage:—"The way men of science shirk these questions is cowardly in the extreme, and shows that their boasted love of truth is only love of themselves and of their little reputations."

The chapter entitled "A Hint or Two about Faith as distinguished from Knowledge," reminds us of M. J. B. Stallo's "Concepts and Theories of Modern Physics," and also of Mr. H. Griffith's "Faith, the Life-Root of Science, Philosophy, Ethics, and Religion."

The "germ theory" is a pet *bête noire* of Mr. Constable. Perhaps, if he were acquainted with the evidence which has lately come to light, he would alter his opinion.

The following passage is worth quoting:—"What strange creatures men are. They run their sewage into the ocean, and then whine that they have no manure; send their rainfall into the gutters, and then whine that they have no water; keep their cattle in unhealthy conditions, and then whine because they become subject to disease; kill and bury them six feet deep, and then whine because they have no stock for their fields; get higher pay for their labour, and then whine because the price of what is produced by that labour becomes higher: insist on and get equality, and then whine because they have thus lost liberty; quarrel with and prevent the formation of capital, and then whine because wages are less than they think they should be, &c."

This is doubtless ably put, but is not Mr. Constable afraid of being called a pessimist—a class for whom he entertains no great affection? By the way, the name "pessimist" is sometimes applied very illogically. A pessimist surely is not a man who sees and says that there is much evil in the world, but one who, inverting the old superstition that everything is right, maintains that everything is wrong and as far wrong as it can be.

Inconsistency is of course to be expected in a work like the one before us. The author defends field sports from the charge of cruelty. Be it so; not being infected with "zoo-folly," we do not bring that charge, and we fully admit and uphold the

necessity for physical culture. But when a sportsman and a champion of sport, thus willing to inflict pain and death upon animals for his amusement, or the cultivation of his own nerves and muscles, ventures to find fault with experimental physiology we are reminded of sundry proverbs not to be quoted in these days.

Were the repetitions and some of the slightly thread-bare anecdotes struck out of these volumes they would become much more readable. In the meantime it is perhaps not unwholesome for "third rate men of science," like ourselves, to learn how we appear from a totally novel point of view.

Portfolios of Drawings and Descriptions of Living Organisms (Animal and Vegetable) illustrative of Fresh-water and Marine Life. Nos. 1 to 8. 1879 to 1882. By THOMAS BOLTON, 57, Newhall Street, Birmingham.

FOR some years past Mr. Bolton has supplied specimens of living organisms for microscopical study, arrangements being made by which they are transmitted by post at periods suiting the requirements of the various institutions and individuals constituting his subscribers. Each tube has been accompanied by a sheet containing a full account of the specimen: these are now issued in a collected form, each portfolio containing about eighteen figures and descriptions.

Respecting the figures they are all truthful, and appear in most instances to be drawn from the objects themselves: when this is not the case the source from whence they are obtained is stated. Although some of the plates are roughly printed, being evidently the work of one of the numerous processes for multiplying copies of writing, and despatched hastily with some specimen unable to bear delay, most are extremely well executed, the whole forming a useful collection for reference, containing figures which would have to be sought for, if published at all, in many costly books and periodicals.

The descriptions, usually on the back of the leaflet, are of great value to the microscopical student, being well adapted to to show the receiver of the specimen what he is to look for,—a needful precaution with a tube containing perhaps a small portion of some plant with numerous all but invisible living organisms attached.

The account is very full, the matter sometimes original; frequently, in the case of a new or rare specimen, a copy of a letter received from some eminent authority to whom it was

sent for identification, and here published for the first time, or else an acknowledged quotation from some periodical or book where the description is to be found.

To criticise the collection as works of art would be unfair, as they are put forward with no claim to that distinction, but are rather to be looked upon as notes taken by the side of the microscope, a state in which an observer's thoughts seldom or never reach the outer world. Natural-history papers would often gain much if the author could put his own work upon the stone, although the result might in some respects compare unfavourably with the work of the professional lithographer; yet a greater amount of truth would reach the reader, as, in spite of the greatest care on the part of the copyist, errors of interpretation are inevitable, and high artistic finish is a poor compensation for want of scientific accuracy.

The series, now to be had apart from the distribution of specimens at a merely nominal price, will prove of great value to those microscopists who study living aquatic forms, especially where access to works of reference is difficult, and will be useful in identifying the results of their pond and shore expeditions.

The Geological Record for 1878. An Account of Works on Geology, Mineralogy, and Palæontology published during the Year, with Supplements for 1874 to 1877. Edited by W. Whittaker, R.A., F.G.S., and W. H. Dalton, F.G.S. London: Taylor and Francis.

The delay in the appearance of this volume need not surprise us if we consider the great and growing mass of journals, transactions, and independent works which the editors have to search through. The sections for America and the Arctic Regions, after being long waited for, have not been sent in by the sub-editor of that department, and consequently the volume has been issued without them.

As regards the first division of the work, *viz.*, Stratigraphical and Descriptive Geology, we cannot help being struck with the small space taken up by researches in Asia, Africa, and Australia, as compared with the British Isles and the European continent. This is, of course, no fault of Mr. Whittaker or of his sub-editors. What has not been done cannot be recorded. But why has so little been done? Why is least done where most remains to be done?

The section on Physical Geology contains brief summaries of many of the more important memoirs indexed. Thus we have an abstract of Sir G. Airy's paper on the probable condition of the

interior of the earth: the author holds that if the planets have been formed by the condensation of nebulæ, the various parts of such nebulæ must have differed greatly from each other. The Rev. Dr. Haughton, in a memoir on a new method of finding limits to the duration of certain geological epochs, gives the minor limit of geological time at the low figure of 11,700,000 years. Mr. W. T. Travers ascribes the warmer climate of the Eocene period to heat radiated out from the interior of the globe. Dr. Streng, in his "Theorie des Plutonismus," considers that as the initial temperature of our earth fell, an upper light layer of molten acid silicates was formed upon a lower dense zone of basic silicates, whilst an intermediate mixture was formed by diffusion, a continued reduction of temperature forming an outer solid shell: the author supposes that the interior may consist of a solid nucleus, with a series of alternate liquid and solid shells between the nucleus and the outer crust. Mr. A. W. Waters, treating of the influence of the position of land and sea upon a shifting of the axis of the earth, considers that any submarine elevation in the oceanic area would displace the pole along the line 52° E. long.

The section on Palæontology is very extensive. There is a notice of Prof. J. A. Allen's paper on a fossil bird from the Florissant shales (*Palæospiza bella*), the feathers of which have been preserved. Prof. W. B. Dawkins, in discussing the British Pleistocene Mammalia, contends that the lion, panther, and urus are the only three mammals exterminated in Europe in the historic period. "The urus breed was introduced into Britain by the English; the fallow deer marks the influence of the Roman power, and the domestic cat intercourse with the south and east of Europe." As regards the fallow deer a difference of opinion prevails. Mr. Grant Allen, in a work reviewed in our September issue, thinks that it formed a part of our indigenous fauna.

Prof. R. Owen, in discussing the "influence of the advent of a higher form of life in modifying the structure of an older and lower form, instances the changes which have taken place in crocodiles since the incoming of terrestrial mammals. In order to prey upon these, along the brink of the rivers, the fore-legs of the modern crocodiles have become longer and stronger, whilst the palato-nares are removed farther back.

The "Geological Record" is an indispensable requisite in the library, not merely of every geologist, but of the student of animal and vegetable life.

Synopsis of the Classification of the Animal Kingdom. By H. Alleyne Nicholson, M.D., Ph.D., D.Sc., F.L.S., F.G.S., &c., Regius Professor of Natural History in the University of Aberdeen. W. Blackwood and Sons: Edinburgh and London.

The author of this little work has undertaken to give simply a skeleton outline of the animal kingdom. He enumerates the sub-kingdoms, classes, orders, and sub-orders; but he has not found it possible to mention even all the families, much less the genera. A number of illustrations are given which are accurately and clearly drawn, with the exception perhaps of the figure of *Cicindela campestris* (p. 72). Definitions are added only in case of the sub-kingdoms or types.

Among the peculiarities of the author's classification we notice that he retains the two Cuvierian orders "Quadrumana" and "Bimana," including in the former the Lemuroidea. He also adheres to Cuvierian principles in placing the Mollusca above, or at least subsequent to, the Annulosa. At the same time we have no right, and certainly no wish, to accuse Dr. Nicholson of conceiving of the animal kingdom as a single linear succession. In a treatise like that before us the representation of divergent series could not be conveniently presented.

Like all the author's former productions, the present work may in the main be conscientiously recommended.

Longman's Magazine. No. I., November, 1882. Longmans and Co.

We have here the first number of a new journal belonging apparently to the same type as the "Contemporary," the "Fortnightly," the "Nineteenth Century," &c. Few of the articles, of course, fall within our special competence. All the papers bear the signatures of their respective authors, according to the modern custom, the British public, it appears, caring less what is said than who says it, and being too slothful to seek out the merits of an article unless enticed by an eminent name.

Prof. Tyndall communicates a paper on "Atoms, Molecules, and Ether Waves," written in his usual clear incisive style. Referring to the celebrated definition of Science as "organised common sense," the writer says that "for the natural history sciences the definition may stand—hardly for the physical and

mathematical sciences." We can scarcely accept this distinction. On the one hand, biology has had great difficulty in disentangling herself from organised nonsense, and, on the other, her most recent conceptions seem to the outsider quite as preposterous as do the deductions of the physicist.

"Our Origin as a Species," by Prof. R. Owen, is somewhat disappointing, as being merely negative in its tendency. The illustrious author still recognises the Cuvierian genera of Quadrumana and Bimana. Now we submit that whatever organ has a heel-bone is not a hand, but a foot, and that consequently the order Quadrumana—four-handers—has no existence.

A story entitled "The Black Poodle" may be a satire on dog-worship.

CORRESPONDENCE.

*** The Editor does not hold himself responsible for statements of facts or opinions expressed in Correspondence, or in Articles bearing the signature of their respective authors.

A FAILURE IN THE PRODUCTION OF HONEY.

To the Editor of the Journal of Science.

SIR,—Bee-masters in the London district, as well as in Buckinghamshire, Northamptonshire, and other parts of the Midlands, complain that their hives have been this season totally unproductive, and that the bees, instead of having any honey to spare, will require food to support them through the winter. Can this be due to some disease among the insects, to a lack of flowers, or to some peculiarity of the weather?—I am, &c.,

APIARIAN.

EFFECTS OF DARKNESS ON ANIMALS.

To the Editor of the Journal of Science.

SIR,—How is it that mice, though spending their lives in absolute darkness, neither lose their colour nor their eyesight? They seem to me to have quite as little need of sight as has the mole, yet disuse of the faculty does not seem to lead into degeneration of the organ.—I am, &c.,

C. G.

NOTES.

Sewage Treatment: the Native Guano Company: Agricultural Show at Aylesbury.—On October 19th a numerous gathering of gentlemen interested in Sanitary Reform and in Agriculture took place at the works of the Native Guano Company at Aylesbury. The display of farm and garden produce, vouched as being grown solely with the "Native Guano," or manure, obtained by the precipitation of the Aylesbury sewage, was exceedingly fine, and was pronounced by practical men most satisfactory. The entries were 441 in number, being above a hundred more than at the last year's show. The purification of the sewage is evidently most successful. We find that some of the most determined opponents of sewage precipitation have overcome their former prejudices. Had even the late Royal Rivers' Pollution Commission been present they would have been compelled to own that the entire process, from beginning to end, is totally free from nuisance.

According to the "Medical Press and Circular" Dr. J. B. Mitchell challenges Anti-Vaccinationists to an *experimentum crucis*. He proposes that a duly qualified person on behalf of the Anti-Vaccinationists shall accompany him to some country where physiological experimentation is free, and that a comparison shall be made between the respective results produced by smallpox inoculation of recently vaccinated and unvaccinated persons. The expenses are to be equally borne by both parties at the outset, but as soon as the decision is arrived at the loser is to defray the whole. These proposals we learn have been communicated to Mr. P. A. Taylor, M.P., and to the "Echo," but in both quarters a solemn silence has been observed.

A medical contemporary writes:—"The Society which is so careful of the few animals which suffer under the hands of scientific investigators, whilst it regards unheeding a millionfold greater cruelties daily occurring, is not dead, but stunned." Surely, then, this is the time to press on with its exposure, so that it may have no time or opportunity for revival!

The Corporation of Wigan have been persuaded into disposing of their sewage by means of an irrigation farm, at a cost of £62,000. A claim is now brought against them for £22,664 12*s.* 7*d.* for the value of the coal underlying half a mile of the line of pipe conveying the sewage to the fields. This is a peril of irrigationism which has not yet been taken into account.

The following chemico-physiological hypothesis, in explanation of some presumed supernatural phenomena, has been suggested to us by a friend whose name, pending further observations, we withhold :—

It is all but absolutely certain that the witch-ointments, magic fumigations, &c., used in antiquity and in the Middle Ages, contained ingredients capable of producing delusions, visions of demons, &c., in persons exposed to their influence. It is certain that the use of the Siberian fungus (*Amanita muscaria*) produces similar effects upon those who swallow it. The suggestion is thrown out that microscopic cryptogams, when inhaled, may produce a temporary derangement of the nervous system, during which the patient fancies he sees, hears, or feels unaccountable sights, sounds, and impressions. Our friend states that when travelling he has sometimes occupied rooms in which he experienced a peculiar uneasiness and a disposition to be continually looking round to see if there was not something unpleasant behind him. A cold shudder down the spine was also experienced. In such rooms there was a persistent odour, faintly pungent, like that of an unfrequented library. He considers that, especially in old castles, halls, &c., the walls, furniture, &c., are covered with minute Fungi, the dust of which, if inhaled, may cause symptoms varying from the slight uneasiness which he has experienced to terrifying visions.

M. Clément Lucas publishes a case in which the production of supernumerary fingers is traced for five successive generations. The starting-points of the families in question, a man and a woman, brother and sister, were both free from deformities. The man was a giant who was exhibited as a curiosity. Of his eighty descendants twenty-four have been affected with deformities of the fingers and toes. This is the more remarkable as there have been no consanguineous marriages among his descendants. In the last generations the deformity appears in the males only. It is to be noted that these deformities become more marked the farther we depart from the original stock. The descendants depart farther and farther from the normal type. (This case is very suggestive from an Evolutionist point of view).

The "Scottish Naturalist" records 137 species of Lepidoptera found in the Orkneys, 59 in Shetland, and 62 in the Outer Hebrides. Only 24 species are common to all the three groups of islands. According to Mr. Jenner Weir the Orkney insects scarcely differ from those of North Scotland. The Outer Hebridean specimens are more differentiated than those of the Orkneys, and their prevailing colour is grey. In the Shetland specimens there is a wider variation in colour, and some of the species diverge into well-marked varieties.

Mr. W. Stokes, in an Address delivered at the Jubilee Meeting of the British Medical Association, says, speaking of the Bestiarians, "Their wrath is reserved not for the sportsman, the gourmet, or the military tyrant, but for the physiologist, who is outlawed if he does not fulfil all the vexatious conditions of an extraordinary Act, the passing of which was simply an insult to our profession."

It is not generally known that the autumnal tints of foliage are not produced in regions where the air is polluted with sulphur-fumes. The leaves shrivel up and blacken without previously turning red or yellow.

A healthy young man, of our acquaintance, has been in some danger from the sting of a single bee, received on the side of the nose near the eye. The roof of the mouth swelled, the œsophagus was for some time entirely closed, and breathing became exceedingly difficult.

The Abbé Moigno says that the ancient cubit, the ten-millionth part of the polar axis of the earth, is preferable, as a standard unity of measurement, to the metre.

Dr. Johnston, of New York, maintains that chloroform cannot be given to sleepers without waking them, save by an expert.

The "Annales de Chimie et de Physique" calls attention to a paper on the "Re-concentration of the Mechanical Energy of the Universe," read by the late Prof. W. J. Macquorn Rankine at the meeting of the British Association in 1852.

A statue in honour of the late A. C. Becquerel was inaugurated on September 24th, at Chatillon-sur-Loing, amidst much speech-making. Prof. Dumas availed himself of the opportunity to glorify beet-root sugar, and to pronounce invention a "quality essentially French."

M. Fremy, "following on the same side," asserted that Daniell's constant-current battery ought to be, and would be, called Becquerel's battery.

Mr. St. Croix Rose ("Zoologist") gives, as an eye-witness, a case of a viper swallowing her young. On cutting her open the young were found in the stomach.

Dr. Tholozan ("Comptes Rendus") in a memoir on the occasional outbursts of plague in Kurdistan, expresses the opinion that the received views on the origin and spread of epidemics, will, in the very near future, have to be submitted to a complete revision.

Every new idea or system, whilst still *sub judice*, is apt to afford shelter and pabulum to a double crop of charlatans; on the one side promoters, on the other side "exposers."

Bee-keeping has this season been a failure throughout most parts of England.

"Les Mondes," in commenting upon the devices of Lahure for preventing collisions at sea, remarks that "in France the fantastic theories of politicians always take precedence of the modest but useful and practical studies of the true benefactors of humanity." In France only?

Anisoplia austriaca, a lamellicorn beetle, has committed this year great ravages in the corn-fields of Southern Russia. It has hitherto resisted all means of destruction.

At the recent meeting of the French Association for the Advancement of Science M. Ayam communicated particulars concerning a case of double-consciousness. A certain Félida X., who has been ill for twenty-three years, has two mental conditions, a normal and an abnormal. In one state she has no recollection of what passes whilst she is in the other.

The death of Frederick Wœhler, for many years Professor of Chemistry at the University of Göttingen, breaks one of our remaining links of personal connection with the science of the earlier half of the century. He was the favourite pupil of Berzelius, and above all his other titles to honour will be remembered as the first chemist who produced an organic compound artificially.

It is strange how the merits of Dr. G. Walker,—"Churchyard Walker" as he was called,—as the father of Sanitary Reform, are ignored. The honour due to him is ascribed to some who shine only by reflected light.

The "Medical News" considers that Carlyle's philosophy was in a great measure the outcome of acid indigestion and flatulence.

According to "L'Union Médicale" there are no rats in the Polynesian Islands, though attempts have been made to acclimatise them, as a favourite food of the natives. Why should they be less able to thrive here than in Mauritius, which has a very similar climate?

The "Boston Journal of Chemistry" notices the extreme tenacity of life shown by weeds and noxious animals.

It is said that the Zulu "medicine-men" who accompanied their sovereign on his late visit to England, when shown rhubarb and ipecacuana,—drugs which are not likely to grow in Zululand,—at once described them as respectively a purgative and as a great vomit-root." Such instinctive knowledge, if shown, *e.g.*, by an ape, would have been the theme for much stilted declamation.

According to the "Salina Journal" (Kansas) a hailstone weighing 80 lbs. (!) was recently found near Salina. Many others, weighing 4 or 5 lbs. each, are said to have fallen on the same occasion.

It is said that only man, the goat, and the wild Carnivora are able to resist the poison of the African tsetsé fly. The supposed immunity of the ass is unfounded.

According to M. Daubrée ("Comptes Rendus") earthquakes have probably their maximum at the new and full moon, and their minimum at the intermediate quarters of the lunation.

The family of the late Prof. Balfour have presented his scientific library to the University of Cambridge for the use of the Morphological Laboratory. A memorial to the dead is in contemplation, but whether it will take the shape of a monument, a scholarship, or a prize for morphological research, is still undecided.

The Victoria Street Bestiarians have issued a reply to Prof. Owen under the absurd title "The Coward Science." To compete with this body either in illogicality or abusiveness would be a hopeless task.

We learn that a literary journal in the Irish tongue will shortly be issued. We wish it success, as every language is an important piece of ethnological evidence, and as such worthy of preservation. It is to be regretted that the ancient Cornish tongue was allowed to become extinct.

Intimations reach us that the "Salvation Army" is to be counted on as hostile to Science in general, and to Evolutionism in particular. Organised ignorance is, even in these days, a power not to be despised.

Prof. Stokes is reported to have said, at the recent Church Congress at Derby, "I prefer resting in the statement of a special creation, without prying into its method." Is this a scientific attitude?

Our worthy contributor Prof. R. Galloway will, like Sir H. Davy, earn the title of a "vera troublesome person in chemistry." He is calling attention to the necessity of adding phosphate of potash to salt meats, as a preventive for sea-scurvy. But officialism clings to lime-juice.

Prof. Galloway has also pointed out the great facilities for obtaining iodine, bromine; and potash from sea-weeds on the west coast of Ireland.

The "American Medical News" chastises Miss F. P. Cobbe for an attack on the medical profession. The "Medical Press and Circular" expresses the hope that "our Transatlantic friends

will not be misled into attaching very serious importance to what Miss Cobbe's prejudices may lead her to express."

The "Lancet" remarks that "to the intelligent student of the medical sciences Berkeleyism supplies the only key that will unlock a world of mystery."

The new Chair of Anatomy and Physiology in the University of Sydney is to be held by Dr. T. P. Anderson-Stuart.

It is announced that a Lecture on "Healthy Motherhood" is to be delivered during the ensuing winter before the Young Men's Christian Association of Glasgow.

Sweeping charges of cowardice are being brought against the London press for having wholly or in part "suppressed" a certain letter from Charles Darwin to a student at Jena. How many scientific or philosophic journals have had the option of "suppressing" it?

MM. Dehérain and Muntz find that nitrates in arable soils are reduced by anærobic organisms, which thus play an inverse part to the ærobic microbia which are the agents of nitrification.